

AMERICAN MEDICAL TIMES

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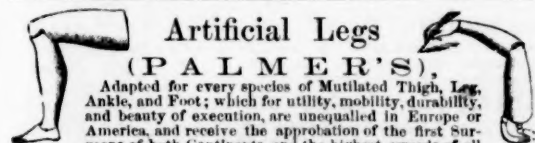
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LECTURES ON AUSCULTATION, PERCUSSION, ETC.

DELIVERED AT THE
BELLEVUE HOSPITAL MEDICAL COLLEGE, DURING THE
PRELIMINARY TERM.

SESSION OF 1861-62.

By AUSTIN FLINT, M.D.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE.

LECTURE V.—PART I.

Mensuration.—Different Modes of Measurement.—Callipers.—Instrument for Measuring Circumferences of the two Sides.—Quain's Stethometer.—Spirometer.—Palpation.—Information Obtained by this Method respecting Tenderness, Elasticity, Prominences, or Depressions, Situation of the Heart's Beat, Aneurismal and other Impulses, Fluctuation, etc.—Normal Vocal Fremitus.

GENTLEMEN:—In my last lecture, I considered the signs obtained by inspection and the application of this method of examination to the diagnosis of diseases affecting the respiratory system. I shall now proceed to consider another method, viz. mensuration, or measurements of the chest. We resort to measurements, or mensuration, for precisely the same objects as we do to inspection, to wit, to determine abnormal variations as regards size and movements. In general, we may obtain sufficient information for diagnosis by inspection alone. The advantages of mensuration consist chiefly in the greater accuracy of the information obtained in some instances, and in the greater precision with which we can express the results of the application of this method. In recording cases, and especially if we intend to analyse or report them, it is often desirable to employ measurements; but, I repeat, we may generally dispense with them in practice. In some cases, as we shall see, the information obtained by the eye is more reliable than that obtained by mensuration.

There are several different applications of mensuration. One application consists in measuring the distances between certain parts on the surface of the chest. A graduated tape or rule answers for this purpose. We desire sometimes to measure the distance between the nipple and the median line, to ascertain precisely how far it is removed when the chest on one side is dilated with liquid, or with liquid and air, and how far it is approximated when the side becomes contracted after recovery from pleurisy. So, it is sometimes desirable to measure the width of the interscapular spaces, in order to determine accurately the increased width in the stage of effusion in pleurisy, or the diminished width after recovery. In recording cases, we may wish to express in figures the situation of the apex beat of the heart in relation to the nipple or median line. The size and situation of local enlargements or depressions may be ascertained with greater accuracy by using the graduated tape or rule, than by estimating by means of the eye. This application of mensuration is of more or less importance.

Another application relates to the diametrical distance between opposite points on the chest. Generally, in this application, the object is to measure the antero-posterior diameters of the chest on the two sides, in order to determine the exact amount of increase or diminution on either side. For this object, instruments are used, called callipers. The instrument which I show you is as convenient and simple as any. It is, in fact, a pair of compasses, with long semicircular branches and a graduated scale near the joint. Suppose we wished to measure and compare the diameters of the two sides at the centre of the infra-clavicular region. We should indicate with ink points in front and behind, on both sides, the points to be equidistant from the median line. We then separate the branches of the instrument and

apply the extremities to the corresponding points in front and behind, being careful to press the ends upon the integument with the same force on both sides, and we note the separation of the branches of the instrument as indicated on the graduated scale. Care must be taken, not only to press the ends of the instrument equally on the two sides, but to measure each side at a corresponding period as regards the respiratory acts, i. e. at the end of an inspiration or expiration; otherwise the comparison will not be fair.

The callipers may be employed to make a comparison of the antero-posterior diameters of the two sides in cases of pleurisy, emphysema, and tuberculosis. They are, however, so far as my observation goes, rarely employed, reliance being placed on inspection.

Another mode of mensuration is to measure the horizontal circumference of the two sides of the chest, for comparison. We may measure each side separately from a spinous process behind, to the median line in front; but it is difficult to make a fair measurement in this way. If we pay no attention to the time of measurement as regards the respiratory acts, and measure one side at the end of an inspiration and the other side at the end of an expiration, the measurement will not be fair. We must cause the patient to hold the breath until both sides have been measured, and this is uncomfortable for the patient, and sometimes even impossible, in cases of disease accompanied by dyspnoea. To obviate this difficulty, we may use the instrument which I now exhibit. It consists of two pieces of inelastic tape attached to a small plate of metal slightly concave, so as to be applied over the spinous processes behind. The two tapes are graduated, indicating inches and fractions of an inch, the enumeration commencing on each tape at the metallic plate. The plate being applied over a spinous process behind, the tapes are brought forward, being careful that each is exactly horizontal in its direction, the median line in front having been marked in ink, and the circumference of each side is observed at the same instant.

If you have not this instrument at hand, it is easy to extemporize one that will answer quite as well, with a little more trouble. A simple piece of tape or cord is passed around the chest in a horizontal direction; a pin is inserted into the tape or cord over the spinous process behind, and the points of union at the median line in front are indicated in a similar manner. The two portions of the tape or cord thus representing the circumferences of the two sides, are then brought together, and the difference in length will show the disparity in size between the two sides. We are to recollect in this application of mensuration, that the size of the right side generally exceeds that of the left side by about half an inch, except in persons who are left-handed.

The semicircular measurement is useful as a means of comparing the size of the chest, from time to time, in cases of pleurisy with effusion, empyema, pneumo-hydrothorax, and emphysema, in this way ascertaining the progress of these affections and the effect of therapeutical measures. This is, perhaps, the most important, in a practical point of view, of the applications of mensuration.

Several instruments for measuring the respiratory movements of the chest have been devised. An instrument contrived by Dr. Sibson is too cumbersome for ordinary use. I hold in my hand a simple and very pretty instrument contrived by Dr. Quain, and called by him the Stethometer. You see that this instrument is of about the size and appearance of a watch. A cord is attached to an interior arrangement by which the degree of movement is indicated by the movement of a hand upon the dial. It is a delicate instrument, but, as it appears to me, it is less reliable than careful examination of the chest by inspection. The cord must be kept in exactly the same amount of tension in measurement of the two sides, else the result is not fair, and it is not easy to comply with this condition. I was formerly accustomed to use this instrument in comparing the superior costal movements on the two sides, with reference to the diagnosis of tubercle, but for several years past

I have preferred to trust to a comparison by means of the eye. If we desire to measure the inferior costal and the abdominal movements, we may employ the tape used in measuring the semi-circumference on the two sides. Measuring each side, successively, at the end of a full inspiration, and a forced expiration, the range of motion is ascertained, and the two sides compared in this regard.

As pertaining to mensuration, I will say a few words respecting the measurement of the amount of air expired from the lungs. Some years since, Dr. Hutchinson, of London, invented an instrument for this purpose, which he called the spirometer, with which he made an extended series of observations. He found that the amount of air which healthy persons are able to expel from the chest by a single act of expiration, differs widely, but the amount bears a certain proportion to the height of the person. The amount is diminished by diseases compromising the respiratory function, and, hence, he proposed this application of mensuration as a valuable means of determining the existence, or otherwise, of disease, and of forming an opinion as to the extent of pulmonary disease. The spirometer has never come into much practical use with physicians, but we often see instruments stationed at public places, and passers-by are invited to test the capacity of their lungs for a small fee. It is not of much practical importance in diagnosis, because in the amount of air which can be expelled by a forced expiratory act, the range in healthy persons is so variable, and we do not generally know the capacity, in this respect, of our patients, when in health. Moreover, various causes, irrespective of pulmonary disease, will affect the ability of prolonging the expiratory act; in fact, any cause which weakens muscular power will have this effect. I shall not, therefore, dwell on this application of mensuration, but content myself with showing you an instrument, made by Coxeter, of London, which is designed to supersede the cumbersome instrument of Dr. Hutchinson.

This instrument consists of a large bag made of India-rubber cloth, into one end of which is attached a stop-cock mounted with a glass mouth-piece. The patient expires into the bag through the mouth-piece, prolonging the expiratory act as much as possible. When the act is finished, the tube is closed by turning the stop-cock. Connected with the large bag, at the other extremity, is a smaller cylindrical bag communicating by means of a tube and stop-cock. At the distal end of the smaller bag is an opening with a stop-cock. The larger bag is the reservoir of the expired air, and the smaller bag is the meter. The latter contains fifty cubic inches, and is graduated so that the quantity of air when it is partially filled may be measured. The amount of expired air having been obtained in the larger bag, the meter is filled by opening the stop-cock, and discharged, successively, until all the air is in this way measured. This instrument admits of being rolled up in a compact form, and is contained in a case which may be carried in the pocket.

This concludes all that I have to say with regard to *Mensuration*. I shall next consider the signs obtained by palpation, or the application of the hand to the chest.

We obtain some important signs by palpation. It will suffice to merely enumerate most of them, their applications to the diagnosis of different affections being sufficiently obvious.

By means of the touch we ascertain the existence of tenderness, its situation and extent. The touch alone, as we have already seen in cases which have been examined before the class, furnishes the diagnostic criterion of intercostal neuralgia, viz. the existence of tenderness in three isolated points, situated as follows: on side of the spinous processes behind, in the intercostal spaces on the lateral surface on one side, and near the median line in front. On the other hand, in pleurisy, pneumonia, and pleurodynia, the tenderness is more or less diffused.

We ascertain by palpation whether the thoracic walls everywhere retain their normal elasticity, or whether, owing to the presence of liquid within the chest, or solidi-

fied lung, or a tumor, an abnormal resistance is felt on making pressure. This sign has already been noticed in treating of percussion.

Examination with the hand of prominences and depressions apparent to the eye, affords information respecting their size and extent.

We ascertain by the touch the situation of the apex beat of the heart, in cases in which the movement is not seen. We ascertain, also, and judge of impulses due to aneurisms or other causes. Cases of empyema have been observed in which the thoracic walls having become greatly attenuated, the movements of the heart, communicated to the pus, overcome an impulse felt over the whole of the affected side. These have been called cases of pulsating empyema. In some cases of empyema, and even of ordinary pleurisy, the chest being much dilated and the walls quite thin, a sense of fluctuation is obtained in the intercostal spaces, the same as when the abdomen is distended with liquid.

By palpation as well as by inspection, and sometimes when the latter method is not available, we ascertain the abnormal divergence of the ribs which occurs in pleurisy with effusion, and in emphysema, and the convergence which occurs when the chest is contracted after recovery from pleurisy. We can also compare, by means of this method, the depth of the intercostal depressions on the two sides.

In addition to the signs just enumerated, which, for the most part, are obtained by other methods, there are some signs peculiar to palpation. Before I can describe these, I must call your attention to the sensation communicated to the hand applied to the healthy chest, when the person speaks.

Applying the palmar surface of the hand to the chest, as I now do in this healthy subject, either at the summit of the chest in front, on the lateral surface, or below the scapula behind, and requesting the person to count in a loud voice, we generally feel a vibration or thrill, and this is called the normal vocal fremitus. It differs in intensity in different persons. The intensity of the sensation depends on the strength and lowness of the voice, on the thinness of the walls of the chest, and other circumstances. In some persons it is wanting. It is comparatively feeble and frequently absent in females. Is it equal on the two sides in health? It is not. This must be recollected. It is always greater on the right side. The disparity between the two sides is often marked, and it may be absent on the left, although present on the right side.

Original Communications.

PROFESSOR N. R. SMITH'S MODE OF REDUCING DISLOCATION OF THE SHOULDER-JOINT.

BY STEPHEN ROGERS, M.D.

OF NEW YORK.

THE subject of dislocation of the shoulder-joint, and its reduction by the procedure recommended and originated by Professor N. R. Smith, of Baltimore, in his article published in the *American Journal of Medical Sciences* for July, 1861, and of which you have already published a summary prepared by Dr. Janes, can scarcely acquire additional interest to the profession from what I may be able to say about it. Small contributions, however, to the very large stock of my early teacher's experience, in this manner of reduction of the dislocation in question, will not, I trust, be unacceptable; instigated as it is by both a desire to add to the favorable record of a valuable surgical operation, as well as to express a pupil's obligation to the distinguished author of the article above referred to. Many years ago, I had the fortune to attend Dr. Smith's lectures on surgery, and to witness in his own hands the reduction of this dislocation,

by what he terms *his* peculiar method. If it be true that it is original with him, I owe the Professor an apology, for having, on repeated occasions, made the statement to my professional associates and acquaintances, that it was first employed by his father, the late Prof. N. Smith, of Yale College. I know not where I received that impression, unless it was from himself.* This, however, is unimportant. More than twelve years have rolled away since, and during that period in a very large public practice, as well as private, I have perhaps had a fair average number of shoulder dislocations; and have invariably employed this method of reduction, and with uniformly good success. They have presented themselves under all varieties of surrounding circumstances; but whatever the advantages or disadvantages, whether in the hut of the poor Indian, who had neither bed nor sheets, or surrounded by abundance of all kinds of apparatus, and material of which to make it, the simplicity of the plan always rendered me independent, confident, and easy. But, it will be asked, what peculiar advantage has the professor made out, for his procedure, over other methods practised by excellent surgeons all over the world: that, for example, of employing the unbooted heel as fulcrum placed in the axilla and adducting the arm down over it; or that of raising the arm, and carrying it up to a line parallel with the vertical line of the body, more or less. It is to be regretted that Dr. Smith has not given us his views upon the comparative advantages of these three plans. His can hardly boast of simplicity, when compared with either of the others. His, in any case, requires more apparatus, or more assistants than either of them. This is a decided objection; what then are the advantages? Is it more generally successful? The Professor may have statistics to answer this query, but I have not. Is it less painful? I have every reason to believe it is. Is it less likely to be attended with injury of the muscles and other tissues about the joint than the others? I believe it is. Is it, on any account, more generally applicable to all the dislocations of this joint, than the other plans? I believe it is. My own cases have been almost always relieved of pain during the greater portion of the time occupied in the extension, the only pain of consequence being at the time of performing any manipulations thought expedient. As an operation it is one remarkably free from pain. This accords, I believe, with Professor Smith's experience. As to the injury to tissues involved, the very fact of its being a comparatively painless operation, should be proof enough. But when we look closely at the anatomical relation of the muscles and bones in dislocation into the axilla, for example, it is at once manifest that if the supra-spinatus muscle is not torn and divided at the moment of the accident, it will be, to a certainty, by the manipulation called *the reduction by the heel in the axilla*; and the long head of the biceps will meet a similar fate, and, of course, involve more or less destruction of the glenoid ligament, and injury to the tissues of the joint. Of the capsular muscles only the teres minor, infra-spinatus, and subscapularis, can possibly escape rupture by this method of reduction. If one or all of these muscles and tendons happen to have been torn by the forces producing the luxation, then the manipulation will be more or less easy, and little serious additional injury can take place in reduction by this method. This heel or fulcrum method, applied to the dislocation into the infra-spinatus fossa, or beneath the coracoid process, is equally objectionable, always resulting in the violent stretching, and generally rupturing, of the tendon of the infra-spinatus, teres minor, and the long head of the biceps in the former case; and the same long head of the biceps, and the subscapularis tendon in the latter; supposing always that the dislocation has taken place without these accidents, which is not always the case.

* Since writing the above, looking over Professor Hamilton's incompressible work on fractures and dislocations, I find that he also has the impression that this method of reduction originated with the late Professor Smith, of Yale College, and that he has given as his authority "Medical and Surg. Memoirs" of Nathan Smith, by N. R. Smith, M.D., his son. May not Prof. Hamilton have taken one of Dr. N. R. Smith's articles for one of his father's, Dr. Nathan Smith; as I believe the book contains articles and cases from both father and son?

The objection to the directly upward extension is, that the more you elevate the arm the greater will be the tension, and disposition to spasmodic contraction, of the three powerful muscles spoken of by Prof. Smith, as so instrumental in the production of the axillary dislocation, viz. the pectoralis major, latissimus dorsi, and teres major; and whose united forces will be in great part exerted upon the already stretched, and perhaps slightly torn tendons, of the supra-spinatus, infra-spinatus, and long head of the biceps, and consequently will, in many cases, probably result in rupture of one or more of them. As in the other case, so in this, if these tendons are torn asunder by the force producing the dislocation, the objection to this particular manipulation no longer exists. In old dislocations, where the head of the bone is more or less fixed in its new situation, this manipulation would evidently be one of the most dangerous, in consequence of the great extension of the soft parts involved, the bound-down and constricted arteries included. As to the more general application to all forms of dislocation of Prof. Smith's plan, I have little more to say. But a single case of the rare dislocations has ever come under my care, yet, as bearing upon the subject of this manipulation, if on no other account, it has a value, and I shall give a concise account of it:—

A laborer of fifty years of age was admitted to hospital after a severe debauch, in a physically sound condition, with the exception of some tremors of the muscles of the extremities, and with the complaint of not having slept for three nights. A few hours after admission he was seized suddenly by a violent epileptic convulsion, which was repeated in quick succession two or three times. They then passed off, and he at once complained of pain of the shoulder, increased upon movement. Placing my hand upon the point of the shoulder, I was struck with the remarkably apparent prominence of the acromion process. I at once removed his clothing, and greatly to my surprise found the head of the humerus resting in the infra-spinatus fossa. The patient's declaration that nothing had ailed the shoulder before he came into the hospital, his not having complained of anything in that region upon admission, in answer to questions put him, and absence of all marks of external violence—which in all probability would have been present in case of such luxation from external force, led me to the conclusion that his was a case of dislocation backwards into the infra-spinatus fossa, and produced by muscular contraction purely.

I have little doubt that the dislocation was produced in the following manner:—The patient lying on his back, the attendant, to keep him in bed, seized the arms, carried them outward till they rested on the bed, at right angles to the body, and there held them immovably—the body meanwhile writhing in all the violence of the convulsion. This position of the arm threw the head of the humerus—as it will be seen always to do—backward into the verge of the glenoid surface, and while in that position a violent spasm of the latissimus dorsi drew it off downwards and backwards, which, with the upward counteracting force of the supra-spinatus, and long head of the biceps, placed it in the position above mentioned, the infra-spinatus fossa. So thoroughly convinced was I of the truth of this explanation of the mode of this dislocation, my invariable instructions have since been to attendants, never to attempt to confine a convulsive patient by fastening the arms. The case being altogether so rare a one, I was induced to allow it to remain some hours unreduced, in order that my colleagues at the hospital might assemble to see it.

When all had examined it, a sheet was passed under the axilla of the injured side, carried up over the body and spine of the scapula, to meet the other end, which passed up in front, over the clavicle; crossing them at this point, they were carried across to the opposite arm, one in front, and the other behind the neck. Counter-extension from both this sheet and from the *hand and forearm* of the well side, and extension from the hand and wrist of the dislocated limb, was kept up for a few minutes, in an exactly

transverse direction, when placing the hand firmly upon the anterior aspect and outer extremity of the clavicle I directed a slightly forward and upward movement of the extended arm to be made; resisting a corresponding movement of the shoulder by counter-force, with my hand as above placed, and the head of the bone almost at once slipped into its place.

Why the sheet was employed at all in this case, I am unable now to recall, as my record makes no comment upon it; but perhaps it was that such an extraordinary case was thought to demand corresponding measures. I have always since had my doubts if it was of any service. I certainly should not employ it now, at least not until the more simple method of extension and counter-extension from the opposite arms and hands had failed. It is important to state that the case above related was a *first dislocation*, not one of those habitually dislocating joints easy to get out and facile to return. All my other cases have been the usual dislocation into the axilla, and I have never employed any other extension and counter-extension than that from the hands and wrists of the opposite arm. As to the additional manipulation, I have practised two—one, the knee in the axilla, and the hand upon the acromion acting counter to it; the other by grasping the arm as near to the head of the bone as possible in the interlocked fingers, with the two thumbs upon the acromion. In this position a grasping force can be exercised much more than sufficient to overcome any obstacle that usually presents to the passage of the head of the bone into its place. This lateral manipulation is, of course, to be employed, only after extension has been fully accomplished. It, as Dr. Smith says, will in many cases not be requisite at all, the bone going to its place as soon as extension has brought out the various muscles to their natural length.

Anatomists hardly require the mechanico-anatomical survey Prof. Smith has made of the parts involved in this luxation; but very properly to brighten those whose anatomical learning has become more or less dimmed by the rusts of time he says: "Let us observe the continuity of ligament, bone, and tendon, by which the two scapulae are bound together and made mechanically dependent upon each other. Anteriorly the two acromion processes are bound together by an unyielding chain of bone and ligament. The fibrous resistant ligaments in this chain are not capable of being stretched. If traction be made from the opposite wrists, the two acromion processes thus tied together are not capable of being drawn asunder to the extent of half an inch." He goes on to say, "Posteriorly the continuity of resisting parts is almost as perfect. The broad expansion of the scapulae approach each other not remotely, and they are bound to each other by the interposition of the ligamentum nuchae, and indirectly by muscles and tendons attached to the spine. Traction from the two extremities will not therefore separate the scapulae to any considerable extent."

For those who are still more curious, I will here give my usual demonstration of the muscular anatomy involved in dislocation of the shoulder-joint, and of course in its reduction, particularly by Prof. Smith's method.

These muscles are physiologically divided into three classes. The first, *those attaching the scapula to the chest or frame*, viz. the trapezius, levator anguli, and rhomboidii posteriorly. The subclavius, pectoralis minor, and serratus magnus anteriorly. The second class are *those attaching the chest to the arm*, and are the clavicular portion of the deltoid—taking the clavicle as a part of the frame—pectoralis major anteriorly, and the latissimus dorsi posteriorly. Third, *those attaching the scapula to the arm*, and are the posterior portion of the deltoid, the coraco-brachialis, teres major and minor, infra-spinatus, supra-spinatus, and subscapularis, and still a single muscle more connecting the scapula and the forearm, the *biceps*. Of the first class, the two great muscles giving the lifting, sustaining power to the shoulder, are directly antagonistic in their action on the scapula, the trapezius drawing backwards and upwards, the serratus mag-

nus downwards and forwards. These two when in action control perfectly the whole expanded portion of the scapula from its neck, and with the pectoralis minor exerting its strength anteriorly and posteriorly from the coracoid process, and the bony and ligamentous connexions through the clavicle at nearly the same point, the scapula is most perfectly fixed to the chest and spine. The remaining muscles of this class are small and unimportant. Now any lateral traction from the arm, tending as it does to separate the scapula from the chest and to sever its attachment to the clavicle, excites to contraction the above-mentioned muscles, and, as one can easily understand, results in fixing that bone firmly to the spine, or central line of the frame. This central line immovably fixed, we have no further difficulty in controlling the scapula, and this of course is accomplished by traction from the opposite arm, which communicates the counter-extending force to the same central points of the frame.

As to the second class, those attaching the chest and arm, their power to dislocate has been described by Professor Smith in these words: "The deltoid violently effects the abduction of the arm, while the pectoral and latissimus, with equal and sudden force, jerk the head downwards into the axilla." He might have added the *teres major* also. But in my experience these muscles are easily overcome by a little steady extension; and if the arm is not raised suddenly, but brought up to the horizontal position slowly, neither traction in that position nor the position itself will excite such spasmodic actions, and thereby endanger the capsular tendons and muscles already described in speaking of the directly upward-traction treatment. As the horizontal position is much within the limits of the extreme relaxed length of these muscles, there can be no physical reason for their opposing the proposed extension for any length of time.

The third class, attaching the scapula to the arm, are, if not torn at the moment of the dislocation, all relaxed by the proposed elevation of the arm to the horizontal posture; and, from the almost plane surface of the glenoid cavity, not one of these muscles can suffer dangerous stretching by the requisite elongation to permit the head of the bone to mount upon the articulating process of the scapula. The muscle likely to give most trouble is the *teres major*; but with the moderate management already proposed for the pectoralis major and latissimus dorsi, will exert an equally calming influence upon this; and as its extreme limit of relaxation is much beyond the horizontal position, it theoretically should give no more trouble than the two muscles just mentioned. My experience in the practice substantiates this theory. The only remaining muscle, and perhaps the most interesting one, in this proposed operation, is the *biceps*. Unlike any of the others, it binds to each other the scapula and the forearm. From this fact it is generally taught and believed that extension of the forearm in any dislocation puts this muscle violently on the stretch. But a careful examination of the anatomy of the part will convince any one that whatever dislocation it may be, simple elevation of the arm will relieve the tension on both heads of this muscle, and that the nearer the head of the bone approaches to its natural position by the proposed extension, the less will be the strain upon them, especially upon the long head, which is the one principally endangered in the accident. Now, as the horizontal or elevated position of the arm is a position of relaxation for the biceps, whatever the position of the forearm, we theoretically would not expect much opposition from this muscle. I believe that the relaxation from this position compensates for the shortening of the long head produced by being drawn out of straight line in consequence of its ligamentous pulley attachments to the groove in the humerus bearing its name. With Professor Smith, I believe that by its contractions it may aid in the restoration of the head of the bone to its place, acting through the ligamentous pulley of the bicipital groove. Theoretically, therefore, it appears to me that Professor Smith's plan of reduction is clearly made

out to be more philosophical, more generally applicable, as well as more agreeable to the patient than any other. Practically my experience is remarkably confirmatory of that of the distinguished Professor. As "the proof of the," etc., I have had no reason to be dissatisfied with this method, and now submit these remarks for the benefit of whom they may concern, as I have already said, with gladness that I am able to give a favorable account of the results of my early lessons learned from the lips of this much-respected teacher.

QUININE AS A PROPHYLACTIC IN MALARIOUS REGIONS.

By J. KING MERRITT, M.D.,

LATE RESIDENT SURGEON TO THE NEW YORK HOSPITAL.

As early as the autumn of 1850, my attention was directed to the use of quinine in tropical malarious districts. I was then acting as surgeon of a mining and exploring party, engaged in operations in the Province of Veraguas, on the Isthmus of Darien. The party consisted of northern whites, of different nationalities, such as Americans, Swiss, Irish, and Germans, and besides, native Indians and Negroes. The locality was a miasmatic section productive of intermittent fever, and its collaterals, of somewhat less intensity and less grave a type than the so called Chagres fever. Every one suffered more or less from frequent attacks of malarious disease. The natives, however, were usually affected with a milder and less persistent grade than the foreigners.

In less than two weeks after my arrival at my post, I was myself attacked with a paroxysm of intermittent fever of ordinary severity. About sixty to seventy grains of quinine were taken during the interval, which produced the ordinary signs, such as ringing in the ears, etc. There was no recurrence of paroxysm, but I continued the use of quinine for seven days afterwards, taking, during this time, from fifty to sixty grains. Notwithstanding this amount of quinine taken into my system, I had a repetition of fever in twenty-one days after the discontinuance of its use, whereupon the same ordeal was adopted of taking quinine to point of saturation after first paroxysm, which succeeded in preventing recurrence of fever as before. These repetitions of fever occurred frequently, although the amount of quinine taken was considerably increased after each attack by the longer continuance of its use. I became anxious, because at every attack of fever I was incapacitated for duty during several days (though having only one paroxysm on each occasion), which resulted from the combined depression of fever and quinine to the point of saturation.

I also observed that several members of the mining staff (Germans), who had been six months in service, were liable to repeated attacks of fever, quite as frequent and as severe as during their first month of service, although at each recurrence of paroxysms of fever quinine had been freely used. Besides, they were rapidly acquiring the malarial cachexia, which had reduced their tone of system much below par, and consequently rendered them subject to protracted convalescence. Upon keeping an account of the amount of quinine, which myself and several of the whites, similarly susceptible to malarious influences, had consumed during two months, I found the average per day, of different individuals, ranged from six to eight grains, and yet all were liable to attacks of fever from over-fatigue or unusual exposure—*particularly to the sun's rays*—whenever the use of quinine had been suspended for some time.

It seemed to me, in my own case especially, that the development of a paroxysm of fever was to be studiously avoided, as, during such, there was a sudden accumulative miasmatic force generated, probably of a zymotic character, which entailed a commensurate increase of quinine to counteract it. Accordingly, I adopted the practice of giving and taking quinine daily, as a prophylactic, in the following manner:—At early morning, immediately before

breakfast, five grains of quinine dissolved in a fluid-drachm of water, by the addition of a sufficient quantity of aromatic sulphuric acid, were given at a dose. This was generally taken diluted in a wineglassful of clear cold coffee, which, by the way, was the best menstruum in my experience to render the quinine acceptable to the stomach, and to disguise its peculiar bitterness so obnoxious to many. At the beginning of this prophylactic practice, it was very often necessary to repeat this dose at nightfall, as premonitory symptoms would manifest themselves, especially after a day of more than ordinary exposure or fatigue; and on such occasions the quinine was administered in a half gill of whiskey, which was used designedly, because stimulation to a moderate degree has proved beneficial in my experience after an exhaustive day. And here I may remark, that it has also been my experience not to stimulate before undertaking an arduous duty; but when the normal powers begin to flag, then the requisite amount of stimulus is very beneficial and most judicious in tropical malarious countries. After pursuing this course for three months, it was very evident that there was a marked improvement in the general condition of the members of the staff before referred to, and in my own case no repetition of fever. In calculating the average consumption of quinine per day for each individual, it was found to be about ten per centum less than in the previous two months. In my own case I now determined, having become somewhat familiar with the premonitory symptoms of malarial disease, which, I believe, can always be detected by an intelligent and close observer, *for they always exist*, to reduce the daily dose of quinine to its minimum. Also, as the members of the party recovered a fair state of health under the daily full doses, I began to try the same experiment with them by gradually reducing the amount of dose rather than the frequency. Every intelligent member of the party, both northerner and native, was instructed in the rationale of the procedure, and urged to promptly report the slightest variation from the standard of health, in order that the dose of quinine might be accommodated. I may here remark that strict sanitary regulations had been enforced from the very commencement of the operations; and although the status of health of the party generally was better than with other similar parties under like circumstances in neighboring districts, nevertheless sanitary measures, irrespective of quinine, and the mode of its administration, were inefficient to prevent malarious diseases from being developed almost universally among the mining staff and operatives.

The experiment for ascertaining the minimum quantity of quinine required as a perfect prophylactic was continued for three months, and resulted as follows:—That in different individuals the prophylactic dose of quinine is not required to vary as much as the curative dose of quinine, and that with the reduction in the aggregate consumption of quinine of sixteen per centum below that of the first two months, the general health of the party had so much improved, especially with the white portion, that only one fifth of the time was lost from sickness, that was formerly the case. There was in my own case even more decidedly favorable results obtained, such as complete immunity from malarious disease, with the reduction of twenty-three per centum of quinine, and the discontinuance of the daily necessity of taking the bitter dose. This relief from the daily use of quinine was also enjoyed by a few other members of the party, who were more than ordinarily cautious and observing, but the rest would be surprised by the stealthy enemy if allowed to act as their own sentinel.

This almost complete immunity from miasmatic disease enjoyed by the party for more than two years, under the prophylactic use of quinine in diminished quantities, was not the result of acclimation, as there was abundant evidence to the contrary in exceptional cases. For example, whenever a detachment of the party on special service was removed from my surveillance, and from carelessness or other circumstances, the use of quinine was suspended for a time, miasmatic maladies were certain to be developed in

the majority. Also, the native, in whom acclimatization must be assumed to be complete, exhibited the benefit of the prophylactic use of quinine, and his dependence on it for immunity from malarial disease, whenever its use was suspended for a greater or less length of time.

In contrast to these results obtained from the prophylactic use of quinine, and to give some idea of the intensity of the miasma pervading this locality, I will mention the case of a gentleman, who became a victim to this scourge of the tropics through a prejudice against quinine. Doctor R—, aged 40, a respectable practitioner of medicine, from spirit of adventure, joined our enterprise. He was enjoying robust health, and was quite prudent in his habits of life. He entertained the notion that the natural powers of his system, with the observance of proper sanitary measures, were able to resist the malarial influences of the country. He was very active and vigorous, and soon became zealously engaged in the mining operations, which gave full recreation to his mind and exercise to the body. In fact, he was a fair example of a hearty man with favorable surroundings and influences to resist malaria. After a month's sojourn, he exhibited the undoubted signs of miasmatic cachexia, and he was warned to use quinine, but would not consent. In the course of the following fortnight these indications increased, when suddenly he was prostrated by a congestive chill from which he never rallied, although heroic measures were then adopted to bring on reaction.

Another illustration of a more extended character I will relate, to show more conclusively the urgent necessity for the use of quinine in this region, and also to give a striking contrast to the results accruing to my party after two years' experience. I will premise, that the mining operations were, after the two years mentioned, disposed of to an English company. A complete mining staff was sent from England to continue the works. It was composed principally of English and Welsh, and numbered twenty-three persons. Everything that science and art could supply was liberally provided in their outfit. Upon the arrival of this party of Europeans, I removed to an adjoining district with an exploring party. The surgeon of the staff was cautioned by myself to practise and enforce strict sanitary measures, particularly the use of quinine as a prophylactic. He partially admitted the necessity, but added, that even if disposed to carry out the plan suggested his superior officers would not sustain him. The result was, that in less than a month there were only five members of the staff, excepting the surgeon, able to perform duty, and three of the five mentioned had taken some quinine at irregular intervals; the benefit of which had been taught to them by previous experience in Brazil and the East Indies. Of the remaining seventeen officers and miners, who had been attacked by the malarial fever, three had died within a week after the paroxysmal development of the disease, and the other fourteen were invalided and sent home, after a month's treatment with full doses of quinine, in a deplorable state of shattered health. In estimating the amount of quinine consumed in the aggregate by the party for the two months' after arrival, and with above-mentioned results, it was found to exceed by a fraction seven grains daily per man. A part of this unhappy result may be ascribed to the free use of stimulants, and negligence of ordinary sanitary measures. These incidents occurred in 1853, and so confirmatory were they of the great necessity to anticipate the explosion of miasmatic impregnations by the introduction of quinine into the system, that it became my settled course of procedure for the six subsequent years, during which I was connected with mining and exploring parties in New Grenada; and it was my good fortune not to have a fatal case or protracted convalescence from malarious disease, either with northerner or native.

In conclusion, my deductions from the experience of nine years' use of quinine in tropical malarious districts are:—1st. That no serious harm to the system ensues from the long continued and judicious use of quinine. 2d. That

quinine given as a *prophylactic*, will certainly prevent the developments of miasmatic disease, and neutralize malaria already in the system. 3d. That the amount of quinine required to maintain a status of health under malarious influences is much less, when used as a prophylactic, than as a curative after development of miasmatic disease. 4th. That the amount of quinine required as a prophylactic is more uniform, than as a remedy after attack of malarial disease. 5th. That quinine will not always restore to health a person after repeated attacks of malarious disease, but will frequently fail to prevent malarial cachexia, especially if not removed from the miasmatic influences. 6th. That cold clear infusion of coffee is the preferable diluent for morning dose, and whiskey for the evening dose of quinine as a prophylactic. 7th. That quinine dissolved in spiritus nitri dulcis produces very happy effects when administered during paroxysms of malarial fever.

OBLIQUE FRACTURE OF THE LEG;

SUCCESSFUL EMPLOYMENT OF MALGAIGNE'S APPARATUS.

By W. L. APPELY, M.D.,

OF SULLIVAN COUNTY, N. Y.

MR. J. I., of Cohecton, N. Y., aged fifty-four years, was thrown from a load of hay, August 26, 1861, and fell forward between the waggon and horses; a wheel passed over the left leg, causing an oblique compound fracture, a little below its middle. The wound was over the anterior surface of the tibia, was six inches or more in length, and the fragments of the fractured tibia protruded. I arrived soon after the accident, and with the assistance of my brother, Dr. J. T. Appley, I reduced the fracture, and laid the limb over a double inclined plane. On examination the next day, the fragments of bone could be felt and seen, the upper fragment overlapping the lower. We then applied the adhesive straps, and made extension and counter-extension upon Dr. Neill's plan and others; but could only keep the fragments in apposition for a short time. I then discovered that a little pressure upon the upper fragment by the finger would keep the fracture reduced.

On referring to Professor Hamilton's work on *Fractures and Dislocations*, I was encouraged to try Malgaigne's apparatus for oblique fractures of the leg. I made the apparatus, with the assistance of a neighboring blacksmith, and I applied it with the assistance and approval of my brother, Sept. 7th, thirteen days after the injury.

The reduction each time caused the patient much pain, but the application of the apparatus caused but little inconvenience. This kept the fractured surfaces of the tibia in complete apposition, and the patient was much pleased to exchange the permanent extension for Malgaigne's apparatus. I used no other appliances after this, except Salter's Cradle; the limb was extended upon the double inclined plane, and swung up clear of bed. I had no difficulty in dressing the limb daily, and after the swelling subsided, the wound healed rapidly. I removed the apparatus Sept. 30th, the patient willing to continue its use, if necessary. Oct. 10th, the patient was able to get out of bed.

I was much pleased with this apparatus in this instance, and think I shall never hesitate to apply it in a case of oblique fracture of the leg, either simple or compound, if I find any difficulty in keeping the fragments in apposition by the usual methods.

CAUSE OF SPINA BIFIDA.—M. Serres explains the production of spina bifida by the theory of arrested development. Certain fossil fishes have permanent spina bifida; and this proves, he tells the Academy, that the lateral portions of the vertebra only are developed, the body of the vertebra retaining its gelatinous form. There are species of fishes now living which present a like condition.—*Brit. Med. Jour.*

Reports of Hospitals.

NEW YORK HOSPITAL.

[Reported by D. B. ST. JOHN ROOSA, House Surgeon.]

TETANUS FROM SLIGHT INJURIES.—ASSAFOETIDA TREATMENT.

It will be observed in these cases that tetanus supervened on comparatively slight injuries, and that the assafoetida treatment received a fair trial, and with no perceptible benefit; the symptoms being scarcely if at all alleviated, the disease progressing to a fatal termination.

CASE I.—Compound Dislocation of Thumb.—William A., æt. 22, Irish, machinist, admitted Aug. 10, 1861 (Dr. Buck, attending surgeon), at 4 P.M. Two hours previously, while in a state of intoxication, fell from a railroad car, producing a compound dislocation of thumb. Patient was unable to state how he fell. He went immediately to a surgeon, who made several attempts to reduce the luxation, but failed. On examination, patient is found to have a dislocation of the distal phalanx of the thumb of right hand. The phalanx protrudes from a lacerated wound. The part is considerably reddened, swollen, and tender. *Treatment.*—Evaporating washes were applied, a slight attempt being made to reduce it by manual extension failing. Sixteen hours after, patient was etherized, and the reduction accomplished after dividing the lateral ligaments. The thumb was flexed, and a bandage applied. Aug. 12th. Bandage was removed, considerable pain and tenderness, wound looked sloughy, poultice applied. 13th. Integuments of phalanx have become gangrenous, bone exposed. Patient's general condition is good, though a little nervous and anxious. 15th. To-day at dinner noticed some slight pain in masticating; no rigidity of jaw. 16th. At 1 A.M., house-surgeon is sent for, and there is found a slight amount of trismus; ordered assafoetida injections, $\frac{3}{4}$ ss. of the emulsion every four hours. At 9 A.M. trismus is more marked, patient is kept in a quiet portion of the ward. Countenance anxious, pulse 118, and at times jerking and irregular. Intellect unclouded; injections are given every half hour; milk-punch and beef-tea are administered as much as may be, though deglutition is becoming difficult, on account of slight spasm. At 11 A.M. trismus is more marked. There is no opisthotonos; swallowing more difficult. 4 P.M. Has taken considerable nourishment since last note; but the spasms of glottis have become much worse, the injections given every two hours. 17th. Patient's mind remained clear all night; opisthotonos supervened, spasms became more frequent. Treatment was persisted in; but at 9 A.M., during a spasm, he died from apnoea. Tetanus supervened on fifth day after injury. Patient survived thirty-two hours after trismus was observed.

CASE II.—Crushed Toe—Tetanus.—John J., æt. 14, Ireland, admitted Sept. 2, 1861 (Dr. Halsted, attending surgeon). Half an hour before admission, toes were crushed by a ferry-boat, his foot hanging over the fender. Great and two adjoining toes fractured, with wounds communicating. Some considerable hemorrhage; no shock; slight compress applied; no dressing. 4th. Patient etherized. Great and two adjoining toes amputated near the centre of the corresponding metatarsal bones, upper portion of the foot being much lacerated. Flap consisted entirely of plantar surface of foot; silk sutures used to unite flap. 6th. Patient's general condition has been fair. Sutures removed this morning on account of tension, and parts left to suppurate and granulate; parts held together by adhesive straps. 7th. Ulcer suppurates freely. Poultice. 10th. Doing well until last evening, when hemorrhage occurred from stump. Patient lost about $\frac{3}{4}$ iv. of blood. Two bleeding points secured, others restrained by prepared lint and peripersulph.; bowels confined. Ordered, B pil. cathart. No. ij. 11th. Bowels not yet moved. Ordered Eecoprotic

mixture. Complains of sore throat; some congestion of tonsils; eats well. Ordered expectorant of ipecac and mucilage. 12th. This morning at half-past eight there is noticed some rigidity of lower jaw: no spasms, and patient swallows without difficulty. Countenance very anxious; pulse 92. Assafoetida injections were ordered early this morning, with stimulants and beef tea, and they are continued every hour; bowels costive. Ordered powder of calomel and jalap. Patient is kept very quiet; all visitors excluded. 13th. Trismus not more marked; has swallowed nourishment and stimulants well, but if not taken very slowly, strangulates; pulse 98, quick and soft. C. 2, assafoetida injections given every three hours. 14th. Symptoms continue; he takes considerable nourishment; opisthotonos present; pulse 100; bowels not yet moved. 15th. Patient began to have severe spasms yesterday afternoon; are now coming on every moment; whole body affected in spasms. 2 P.M. died.

CASE III.—Compound Fracture of both Bones of Leg—Tetanus.—James P., 45, Wales, seaman, admitted May 16, 1861 (Dr. Markoe). Ten days prior to admission received a compound fracture of both bones of right leg. To bring parts into apposition, one inch of upper fragment of tibia was removed. Limb placed on a pillow, and a bandage applied. On admission parts about fracture are in a state of moist gangrene, a granulating ulcer about one inch in diameter communicates with fracture situated at about the junction of upper with middle third. Pulse 164. Tongue furred, brown in centre, white at edges. Bowels costive. Dressings have always seemed tight, and continues thus, causing considerable pain. Since placed in fracture-box, all tight dressings removed, and antiseptics applied. 18th. At 4 A.M. trismus manifested itself. Ordered, R. assafoetida gr. iij. Ft. pil. No. xx.; one every two hours; also, R. Mag. Sol. Morph. Sulph., grs. x., every three hours; beef tea, milk punch. 19th. Opisthotonos present. Spasms often occurring. Has taken very little nourishment; at 6 A.M. died. Inception of tetanus twelve days after injury. Death occurred twenty-six hours after.

The attending surgeons in this hospital are unremitting in their warnings against *too much dressing*, in the way of bandages, the temptation to which is so strong, when handsome reverses, in bandaged limbs, are so highly regarded. The case above noted is one in which we may be justly suspicious that more attention was paid to bandaging the limb than curing the fracture.

No post-mortem examination was obtained of the foregoing cases.

AMERICAN MEDICAL JOURNALS.

THE CHICAGO MEDICAL JOURNAL.

I. PURULENT OPHTHALMIA OF ARMIES. By E. W. HOLMES, M.D., of CHICAGO. II. CASE OF AMPUTATION OF LEG FOR LARGE SLOUGHING ULCER OF HEEL AND ANKLE. By L. B. BROWN, M.D., of IROQUOIS, ILL.

1. The intention of Dr. Holmes in this article seems to be to call the attention of military surgeons to the treatment of the disease by strong solutions of nitrate of silver (3j. to 3ijss. to 3j. water) applied freely to the conjunctiva.

2. Dr. Brown's patient was 66 years of age. He injured his ankle by jumping from a carriage. A large sloughing ulcer of the heel followed, which extending, laid bare the os calcis and lower portions of tibia and fibula. Amputation was resorted to at the point of election: the result was very satisfactory.

PHOTOGRAPHY IN MEDICINE.—M. Gerlach of Erlangen announces that he has found out the way to obtain very perfect photographic copies of microscopic objects. As there appears to be no convenient method yet known for preserving animal microscopic specimens, this discovery, if confirmed, must be considered a grand one.—*Brit. Med. Jour.*

American Medical Times.

SATURDAY, NOVEMBER 9, 1861.

ETHER AS AN ANÆSTHETIC.

WHEN, some time since, we had occasion to allude to the fact that ether had proved fatal in a number of instances, the announcement was received with much surprise, and more than one incredulous correspondent was moved to inquire on what authority the statement was made. The authority was given, and reference was made to twenty-five cases that up to that time had been reported. The Boston Society for Medical Improvement soon after appointed a committee to report "on the alleged dangers which accompany the inhalation of the vapor of sulphuric ether," and recently this committee discharged their obligations in a lengthy and very elaborate report. That they have had "unequalled facilities" for collecting material this document bears the most unequivocal evidence; and in this respect it may be considered as complete. The report, the text of which appears on another page of this number, is copied from the *Boston Med. and Surg. Jour.* of Oct. 24, and consists of two portions, the text and an appendix of forty-one cases of alleged deaths by ether, upon which the conclusions of the committee are based. The profession is under great obligations to the Boston Society for instituting this investigation, and to the committee for the unwearied zeal they have manifested in the collection of evidence upon the subject of their report.

The questions to which anæsthetics and their uses have given rise, are all of great practical interest, and deserve, as they have received, the most thorough discussion. These agents will never be discarded; and it only remains to be seen, which will finally, after prolonged trial, gain the confidence of the profession. The present position of the two agents, ether and chloroform, may be thus stated:—Ether is slow in its action, requires a large bulk, is disagreeable generally to patients, but is rarely fatal; chloroform acts promptly, in small quantities, is agreeable, but is frequently fatal. The extreme partisans of these agents will doubtless take exceptions to this statement, for it is asserted, on the one hand, that ether acts as promptly as chloroform, is as agreeable, and is *never* fatal; and on the other, Dr. SIMPSON recently stated that he had seen ill effects from chloroform but twice, and these were slight. Such partisanship in the discussion of a question which is to be determined by observation and experience, is to be deprecated, as it tends to lead astray those who have not large facilities for observation. We must receive with many grains of allowance the conclusions of writers whose opinions we know are preconceived, and should never adopt them without the most rigid scrutiny of the facts on which they are based. Dr. SIMPSON's assertion of the innocuousness of chloroform will weigh as dust in the balance with those who have witnessed the grave consequences which occasionally follow its administration. And we can but consider it unfortunate in this single particular, without in the slightest way reflecting upon the candor of the committee, that the members of the commission to investigate the

alleged dangers of ether, are all residents of Boston, a city which is justly proud of the distinction of giving ether to the world. We could not, on this very account, avoid the curiosity of examining critically the appendix of cases, in connexion with the references, to see how far they sustained the conclusions. We must confess to not a little surprise that some of these conclusions are so positively stated; and we cannot, in justice, pass them without a word of comment.

Although much of the report is occupied with a discussion of the comparative merits of ether and chloroform, with a leaning towards the former not always founded upon an impartial judgment, the real subject proposed for the consideration of the committee was, "the alleged dangers which accompany the inhalation" of ether. And this is the point of chief interest to the profession, viz.:—Has ether ever resulted fatally when used as an anæsthetic? To give the reader a fair opportunity to decide this question for himself, we copy from the appendix of the report the following cases of alleged death from ether.

"1. Hotel Dieu of Auxerre, France, 1847. A man, fifty-five years old, was operated on for cancer of the breast, after having breathed sulphuric ether, and died during the operation with evident symptoms of asphyxia. The ether was inhaled from a Charrière's apparatus. 'The want of care in administering the ether, which was given in a manner likely to produce asphyxia, and the insufficient means used for the restoration of the patient, sufficiently explain the cause of death.'

"2. Mr. Robbs, Grantham, Eng., 1847. Ann Parkinson, aged 21, inhaled sulphuric ether several times, experimentally, preparatory to an operation for removing a large malignant tumor in the soft parts of the thigh. At the first trial, after breathing fifteen or twenty minutes, she became hysterical and comatose for two hours; at the second trial she became comatose in four minutes, recovering rapidly. The next day she inhaled it for ten minutes prior to the operation, which lasted half an hour, and during which she manifested great pain, declaring afterwards that she felt every cut that was made; she was in a state of great exhaustion when the operation was completed, being obliged to have brandy before it was quite over, and more when she was put to bed. She was conscious from the time of the operation to her decease, forty hours afterwards, but spoke in a low, faint voice. All the natural functions were sluggish. At the autopsy it was found that there was some congestion of the brain and lungs, and the blood was fluid. There were no other unusual appearances.

"8. Roel, Madrid, 1847. Dolores Lopez, an intemperate female, aged 50, is in a condition of hectic and has shortness of breath. Inhalation of sulphuric ether for the removal of a scirrhus tumor of the breast, weighing three and a quarter pounds. The anæsthesia was incomplete, though the pain of the operation was slight. The loss of blood was not great. From ten o'clock, when she was etherized, till half past twelve o'clock, she remained stupid. Soon afterwards the pulse became thready, and there was sub-delirium. In about eight hours from the operation she died. At the autopsy, the firm, arborized brain was found infiltrated with serum both externally and internally. Sinuses full. The lungs show signs of old trouble, and are partially congested. Blood, fluid.

"12. Mendoza, Barcelona, Spain, 1847. A female, aged 60, underwent amputation of the leg, three days after suffering a comminuted fracture of the foot, which had been crushed by a wheel. Sulphuric ether was given, but it caused great uneasiness and cough; insensibility was, however, obtained at the end of a quarter of an hour, but it lasted only a few minutes. After attempts at re-etherization had been continued for half an hour, an hour was given her to recover her strength. Then she breathed it again,

and was put to sleep in six minutes. She called out a little when the skin was cut, but afterwards remained silent. Although the pulse was full, the patient's exhaustion was manifest, and after the ligatures were applied she had a violent nervous attack, which was overcome by stimulants and the admission of fresh air. She then lay quiet, but exhausted, answering questions with a weak voice, though remembering nothing which had passed. This forgetfulness persisted, she became stupid, with a weak pulse, and heavy and stertorous breathing; her strength failed more and more, and she died fifteen hours after the amputation.

"19. M. Barriere, Lyons, France, 1852. A female, aged 55, in a weak and bad state of general health, underwent excision of the superior maxillary bone. Sulphuric ether was given from a sponge placed in a bladder. Death occurred during the operation. It was thought possible that it might have been due to the hemorrhage.

"20. Dr. G. de Oettingen, Dorpat, Russia, 1847. Constantly increasing anemia, on account of a gangrenous ulceration of the leg, rendered amputation necessary in the case of an old man, seventy years of age. Sulphuric ether was administered from a common bottle, having an opening large enough to include the nose and mouth of the patient. The operation was completed, but the arteries were not tied when the indications of approaching death were noticed, which shortly occurred with symptoms of syncope. There was no hemorrhage. The autopsy gave no explanation of the death.

"23. Dr. —, New York, 1860. 'A patient with hernia had been laboring under symptoms of strangulation for some time, and was in a desperate condition. A cutting operation for his relief was resolved upon. He was fully anesthetized with sulphuric ether, and suddenly, during the progress of the operation, showed symptoms of prostration and soon died.'

"Dr. W. H. Mussey, Cincinnati, O. A man, 50 years old, by the upsetting of his wagon, had been dragged thirty feet, and then rolled down an embankment eight feet high, when his wife, weighing 205 lbs., fell upon him. The patient, himself weighing 230 lbs., was occasionally intemperate, having had a debauch ten days previous to his accident, and had been quite sick in recovering from it. After his injury he had to be transported a mile in order to reach his house, and during the thirty hours ensuing he suffered intense pain, and took four grains of morphine in divided doses. In order to examine an injury which had befallen his hip, four ounces of sulphuric ether (it being the unanimous opinion of those present that his condition would not permit the use of chloroform) were administered with great care and precaution. 'At the time of seizing the limb for examination'—we quote from Dr. Mussey's own account—'a peculiar shortness of breath of an asthmatic character was noticed, and it was stated that he was subject to attacks of asthma. On observing this phenomenon, the use of ether was suspended and not resumed; the manipulation was, however, proceeded with, the patient screaming out and writhing with pain, and apparently perfectly conscious. Seeing that his lips were perfectly purplish and his breathing very short, I proceeded to administer for his relief. He called for water; a little was given him, and a little vinegar was put in it; finally, some whiskey was procured and administered in warm water. But little, however, was taken. The patient complained that he was suffocating, and the tongue was drawn out, though there was no lack of control of it, as he put it out to take stimulants. He was rolled upon his side; water was thrown in his face; first cold, then hot water applied to his forehead; the Marshall Hall method of artificial respiration, and the additional one of inflating the lungs from my own lungs, with forced expulsion of air, and flagellation of buttocks, were continued from fifteen to twenty minutes, when the patient was abandoned as dead.' At the autopsy, a space six inches in diameter in the right iliac and lumbar region was found blackened and purple with extravasated blood, and this extended anteriorly and superiorly upon the wall of the

abdomen. It also extended through the entire pelvic cavity, and beneath the pelvic fascia there was a large deposit of blood. The source from which this emanated was a most extensive fracture (which in fact might be called comminuted) of the os innominatum, radiating in all directions from the acetabulum to the circumference of the bone. There was a large amount of adipose tissue upon and around the pericardium, and fatty deposits on the auricles of the heart, which had no structural disease other than an absence of the usual redness and firmness of the tissue. Nothing of importance presented itself in the rest of the examination.

"25. Dr. —, New York, 1860. 'A very large, old scrotal hernia had from some cause become irreducible. Inhalation of sulphuric ether was resorted to, and, while the patient was under its full influence, the hips being raised and the head allowed to be forcibly flexed upon the chest, the taxis was resorted to. The large mass of intestines very suddenly receded into the abdomen, and just at the moment the patient was noticed to be in a dying condition, from which he could not be recovered.

"27. Dr. J. Y. Bassett, Alabama, 1847. It was proposed to apply the actual cautery in a case of tetanus. Sulphuric ether was administered by a dentist. 'At this time the patient's pulse was good, and there were no signs of an immediate extinction of life. In one minute the patient was under its influence; in a quarter more he was dead—beyond all my efforts to produce artificial respiration or to restore life. All present thought he died from inhaling ether.

"30. Mass. General Hospital, 1855. A man aged 32, much addicted to drinking, sustained a compound fracture of the left leg, the tibia protruding an inch. Five days after the accident, delirium tremens appeared. On the second day of the attack, the patient's wife was told that he could not live. He was exhausted, bathed in perspiration, and had a feeble and rapid pulse. His delirium was such that the house pupil undertook to etherize him. He made the usual struggles, and had some opisthotonic spasms. The ether had been continued some minutes, when the breathing was noticed to be abdominal, although the pulse was quick and sufficiently strong. Within a quarter or half a minute, the pulse suddenly ceased; the lips were not blue, and the head and hands were warm. The patient was dead, and no efforts to restore life were of avail. At the autopsy, the sub-arachnoid fluid was found in larger amount than usual. Nothing remarkable about the brain. The heart was soft and flaccid, and contained some yellow, gelatinous coagula in the right side, and a small quantity of fluid blood in the left. There was no valvular disease. The liver was fatty. The kidneys and other organs were healthy."

It is quite impossible to read such cases as these without being impressed with the fact that ether is not altogether incapable of harm, even when cautiously administered. If the word chloroform were substituted for ether in these cases, most of the deaths would be placed to its account. There are few who will not now acknowledge the unquestionably and unavoidably fatal effects of ether in a given number of cases, however we may attempt to account for the death by other causes. Here are sudden deaths during the inhalation of ether without any autopsical appearances to explain the result. The refinement of speculation which would still refer the death to some undiscovered cause, rather than to the anæsthetic, would readily explain away all the alleged deaths from chloroform.

The committee require that death, to be chargeable to ether, shall occur during the anæsthetic state, and that it shall be inexplicable by any phenomena of disease or operation. This restriction excludes a number of deaths occurring several days after the use of the ether, and manifestly due to arachnitis excited by its inhalation. Three cases

are reported where ether was inhaled several times for amusement, resulting in meningitis, and death. The committee charitably absolve "the anaesthetic from any responsibility," but significantly add: "It is not surprising that an intoxication, such as results from ether, should, especially in children, lead to a fatal issue in the manner described."

In conclusion, we must again commend the efforts of the committee to render their report complete. It is one of the most important medical papers which has appeared during the year, and we hope it will have a wide circulation in the profession. It will do good by opening the eyes of medical men to the dangers of the careless and indiscriminate use of anaesthetics. Of the danger of chloroform and of its fatal effects no intelligent practitioner has for a long time doubted; of the *not entire innocuousness* of ether no one who peruses this report will longer doubt. Every candid and thoughtful reader must conclude that anaesthetics of every description are powerful and dangerous agents, which are always to be used with great care and discretion.

THE WEEK.

PHYSICIANS to our public institutions, especially Dispensaries, are frequently shocked with the tales of crime and seduction which the recently arrived emigrants relate that they have witnessed on board of emigrant ships. In some instances these vessels are nothing less than floating brothels. Hundreds of young girls come to this country, unprotected, to meet friends who have gone before them; simple, unaccustomed to the world, and thrown for weeks directly into the toils of designing men, they readily fall into habits of the grossest and most unblushing licentiousness. Arriving in New York without means of support, they are immediately beset by "runners" for houses of low repute, and readily yield to the proffered rewards. The efficient agents in this system of female demoralization, and recruiting for the brothels of New York, are the crews of the emigrant vessels. It is stated on the best authority that seamen engage in these ships solely for the inducements for unchecked and unlimited licentiousness. The only remedy for this monstrous evil is in the selection of moral crews for our merchant service, and this can only be done effectually by concerted action among shipping merchants. A society has recently been formed in this city called "The American Shipmasters' Association," which is designed "to elevate the moral character and professional capacity of seamen, by the encouragement of worthy and well qualified officers." The Society was called into existence by the great annual loss of life and property in the American merchant service, owing to the unqualified seamen. Let them extend their inquiries, beyond the nautical qualifications of officers, to their moral characters, and they will strike a blow at one of the most disgraceful systems of crime ever organized.

WHEN religion seeks to advance the physical welfare of a people by giving its solemn sanction to the principles of sanitary science, it never fails of accomplishing its purpose. Men will listen to the voice of a religious monitor, and implicitly obey his instructions, when they would be indifferent if the same advice came from any other source, and even appealed to their reason and common sense. The example of the great lawgiver of the Jews is an illustration of what power religion has of enforcing its precepts.

Many of the laws which were enacted with such solemnity for this nation, and religiously enjoined upon each family, were but simple expressions of great sanitary truths which are as much revered by that people to-day as when first promulgated. Clergymen rarely use the power they have to enforce the simple but highly important laws of correct living, partly doubtless through personal ignorance of what those laws are, and partly from a mistaken apprehension of the precise limits of their duties. We are glad to record an exception to this almost universal neglect of the physical interests of their parishioners by clergymen. BISHOP TIMON, of Buffalo, N. Y., a most intelligent and excellent Catholic bishop, has addressed a letter "to the honored and pious Christian women of the diocese," on the impropriety of low-necked dresses. He considers his subject both from a sanitary and a moral point of view, and derives from each sufficient reasons for enjoining upon them a change in this regard. The following is his emphatic exhortation:—

"But whatever may be the sentiment of the learned and the wise, on the danger of the low-necked dresses to the health; and whatever may be our wishes for the temporal happiness of the Christian women in our diocese; and whatever our zeal for the sacred mission, which women have to cultivate from earliest youth, and form, as only a mother can, the Christian life and spirit, in their sons and daughters; yet we dare not press upon them in the relations of society those rules of prudence, when they or their children prefer to wear fashionable, low-necked dresses in fashionable circles. But we most earnestly exhort all ladies, the very young as well as those of more mature age, not to appear in church, nor assist at Catholic sacred functions, nor present themselves for the reception of the sacraments, without having the neck, shoulders, and breast modestly covered. And we request all pastors of souls, and all religious ladies engaged in teaching, to use every possible exertion and influence to see that this advice be accepted in the spirit of charity, and of zeal for that which best pleases God, with which it is offered."

AN interesting fact relating to deaf mutes was recently noticed by DR. DETMOLD, of this city. Being called to a child which a physician had pronounced idiotic, because it had not spoken, except the words "mamma" and "papa," on examination he discovered that it was a deaf mute. The following ingenious explanation of the speech in such cases is well worthy of attention:—

"The ordinary attempts to find out such cases are very apt to fail, and it requires some little ingenuity to prevent mistakes. The great difficulty seems to be that the children have *talked*, and I have never yet, among a large number that I have seen, met with a single case of deaf mutism that has not talked; and that is a point to which I wish to refer. If physicians will pay attention to it, and ask the mother of the deaf mute child, did the child talk? the mother will always say that her child has said some words, but they are invariably the same. All deaf mute children, I assert—I do not believe that there is an exception, at least I have seen none—have said 'mamma' and 'papa.' At first when I made this observation it somewhat staggered me. The parents and physicians thought that the child, having spoken, must talk, and that some late occurrence, for instance, scarlet fever, had interfered with the hearing. But upon reviewing the cases, and finding children all saying the same thing, I looked for another solution of the question, and I think I have found it. We know that speech may be carried on in two different ways; also one ordinary way in which children learn to speak is, if I may express myself so, by *experiment*; the child hears a sound and tries to imitate it, he keeps on until he has succeeded, and then he begins to speak. But there is another way that I might,

as an antithesis, call *theoretical*, and that is best illustrated by the new method by which the deaf mutes are taught in Europe actually to articulate. On the continent there are now schools for deaf mutes, where they learn to speak in the following manner: Starting with the principle that speech is brought about by the action of certain muscles, the deaf mutes are taught to train their muscles by imitation, and speech, as a matter of course, will follow. The teacher, therefore, stands with his neck bare before the deaf mute, and shows the motions of the muscles of the throat and lips in such a way as that they can be imitated. It is an established fact that articulation is taught in the schools in Germany, and some years ago there were a number of teachers sent from here to Europe to investigate the matter, and although it had no great practical result, yet it had a scientific value in explaining why deaf mutes can talk. Now if you look to what they have said, you will find, as I have before remarked, that the same words are used in every case, and these are altogether labial sounds; sounds which are produced altogether by the motions which the child has seen and learned. 'Mamma' and 'papa' are the sounds which the mother takes special pains in the beginning to teach the child. The child, in its turn, being cut off from hearing, that most important means of communicating with the outer world, has a very acute observation, sees the mother making those motions, he imitates them, and finding the mother pleased with his efforts, at last is able to articulate the words. When, however, the mother ceases to teach the child in that way, and when it becomes more attentive to other motions, he forgets his first lessons. Let me add another remark, which is not without interest, and that is, I have found that in all languages, no matter whether civilized or savage, in all tribes, and in all nations, the relationship of parent to child is expressed by simple labial sounds similar to 'mamma' and 'papa.' There is no nation which expresses such a relationship by a guttural sound, or by any sound which is not easily imitated."

We are accustomed to regard Spain as very much behind other continental nations in its appreciation of medicine and medical men. And yet her laws are in many respects well worthy of our imitation. Take the following relating to pharmacy:—

"Spanish physicians and surgeons never dispense medicines or engage in pharmacy. Should, however, any person so licensed assume the latter calling, by existing laws affecting pharmacæans, he would forfeit his professional privileges, while those keeping shops for supplying drugs prescribed by the former classes—no pueden ejercer simultaneamente la medicina ni la cirugía—cannot exercise simultaneously medicine or surgery. On that point there exists no equivocation. Further, pharmacæans cannot sell a secret remedy, nor any special or specific preparation whose composition is unknown. Should it ever happen that the prescription of an unauthorized medical practitioner orders strong medicines, especially if poisonous, in extraordinary or unusual quantity, before compounding such doses the pharmacist must first communicate with the prescriber to verify his recipe, lest the party may have made a mistake."

POISONING.—Poisoning by laudanum is becoming rather fashionable in Melbourne. Within the last month five or six cases of poisoning have been admitted into the Hospital; several other cases have been recorded in different publications. It is a matter for serious consideration, whether druggists ought to be allowed to sell large quantities of laudanum or opium, or not, to any person who chooses in a fit of semi-insanity to walk in and ask for either. If the Legislature does not interfere, to prevent druggists selling large quantities of poison, people will soon begin to try their hands on each other.—*Med. Record.*

SURGEONS OF CALIFORNIA REGIMENTS.—DR. REID of Stockton; DR. WILLIAMSON and McNULTY of San Francisco.

REPORT OF A COMMITTEE
OF THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT,
ON THE ALLEGED DANGERS
WHICH ACCOMPANY THE
INHALATION OF THE VAPOR OF SULPHURIC ETHER.

ANY one who has observed the course of events, especially the tone of journals and the published statements of late surgical writers, as Erichsen, Dr. Hamilton, and others, must have noticed a diminishing confidence in the safety of chloroform and an increased willingness to allow the greater security of ether. Various influences have, however, prevented the disuse of the former, even by many of those in whose hands accidents have occurred, and it still remains the anæsthetic most in vogue. When the subject of chloroform first came under discussion, its dangers were commented upon, and even then freely acknowledged. It had not been two months introduced when "a well developed girl of fifteen" died from its administration for the evulsion of a toe-nail, "the process of inhalation, operation, and death, not having occupied more than five minutes."* Since that time deaths from its use have repeatedly occurred. On the other hand, fatal results from ether, although still figuring in the statistics of mortality from anæsthetics, are everywhere admitted to be very infrequent. Indeed, the opinion has been expressed by various authorities, both in America and Europe, that a death really attributable to the inhalation of sulphuric ether is yet to be reported. The correctness of this opinion has, however, been repeatedly denied, and the strong conviction of the absolute safety of this agent, which exists in some localities in this country, is thought to have its foundation rather in the desire that the fact might be established than in the proof that it was so. Of course no one intends to say that a person cannot be killed by ether. The inhalation of its vapor, without a sufficient admixture of oxygen, destroys life by asphyxia. This may happen, and unfortunately has happened, but such an event cannot be laid to the anæsthetic, since, in such a case, it is the method of administration, and in no sense the ether, which causes the fatal result. It is the purpose of this report to solve the doubt just implied with regard to the absolute safety of sulphuric ether, and to investigate the dangers of its use as compared with chloroform. In pursuance of this object, therefore, we propose, in the first place, to consider what conditions and precautions are necessary in bringing about a state of insensibility by its use, and what phenomena of etherization have an apparent or real danger.

1. The safe inhalation of ether requires proper attention—1st, to the quality of the article used; 2d, to the method of administration; 3d, to the symptoms which present themselves while the patient is under its influence.

1st. *Quality of Ether.*—Ether for inhalation should be of unquestionable purity. A large amount of inferior ether is sold which cannot readily be distinguished from that which is pure, except by its effects, although an expert, familiar with its properties, may infer something from the odor and other sensible qualities. The inferiority may be due to oxidation from bad corking, the presence of alcohol, sulphurous acid which has not been removed by thorough washing, and volatile oils. Either of these impurities may give rise to a tedious and imperfect inhalation, and the latter of them, by irritating the bronchial mucous membrane, to such coughing, struggles, and resistance, that the patient is finally etherized in a distressing and unsatisfactory manner. Accidents of this kind lead to a disparagement of the value and practical usefulness of ether. It is therefore an advantage for the surgeon to procure his own ether, or to use from an "original package," of the character of which he has already assured himself. There are two brands of ether in common use in this country, viz. that manufactured by Powers and Weightman of Philadelphia, and that by Dr. Squibb, of Brooklyn, N.Y. These are uniformly of excellent quality. The latter is remarkably anhydrous, but possesses an

* Medical Gazette, Feb. 11, 1848, p. 255.

odor more harsh, disagreeable, and intensely etherlike than the former, and, in the opinion of those who have used it extensively, produces more choking during inhalation. This may be remedied to a certain extent by moistening the sponge from which it is given in water, enough of which will perhaps be taken up by the ether to diminish its unpleasant effects.

Ether may be made purer by simple agitation in lime-water, allowing the water to settle, and then decanting; and this washing is practically, and for general application, as good a method of purification as can be adopted without re-distillation.

2d. Method of Administration.—Ether should never be given from any inhaling apparatus. The best medium of its administration is a bell-shaped sponge, large enough to cover in the nose, mouth, and chin; but it is difficult to find one of sufficient size and close enough in texture, or without such numerous apertures at the root as to admit too freely the atmospheric air. A sponge of this sort, moreover, being as expensive as rare, is seldom used outside of hospitals. A stiff towel, properly folded, may be substituted, and has the advantage of being always at hand; as it may be left behind, the surgeon does not carry away with him the annoying odor of an impregnated sponge. It is desirable that the towel should be a new one, and of pretty good size. It is to be taken just as it comes from the laundry, and not unfolded further than to display it in the dimensions of about ten inches by five; by folding down two of the corners in such a way that they shall lap over each other a little, and securing them by stout pins, a cone will be made which fits the face admirably. The thick layers of towelling will hold sufficient ether, and its texture prevent a too free dilution of the anæsthetic by the atmospheric air, provided the apex and seam of the cone are carefully and tightly closed, either by pins or the fingers. As the cone becomes collapsed by saturation, it should from time to time be opened, and kept in shape by distending it with the hand. Unless these details are attended to, and especially the closure of the apex of the cone, the induction of anæsthesia will be uncertain and protracted. In anything so porous as a towel or sponge, the difficulty is to exclude enough air; for while its adequate admission to the lungs during etherization is essential to the life of the patient, its too free entrance not only delays anæsthesia, but induces a condition of excitement, both mental and physical. The importance of excluding the air, as above stated, is a point not generally appreciated, but the necessity of it has long been known to those most accustomed to the use of ether, as shown by the "chemise" with which, in hospital practice, a too porous sponge is often covered to expedite the etherization of a rebellious patient. Ether should be poured lavishly on the towel or sponge, an ounce or two at a time, especially at the commencement of inhalation. Although it may be wasted, too much, so far as safety is concerned, cannot be used. A small quantity poured on hesitatingly and timidly, as is sometimes done, has the same effect as a too free dilution of the vapor with air, producing simply intoxication and its accompanying excitement without anæsthesia; whereas a large amount, though the cough and choking sensation which the greater volume of vapour produces may cause the patient to resist and struggle, is certain to bring about a satisfactory condition of insensibility.

3d. Phenomena of Etherization.—A strong, full-blooded man is pretty sure to resist the approaches of anæsthesia under any circumstances. This may sometimes be overcome by warning him beforehand of such a possibility, and inducing him to resolve not to struggle; the last impression on his mind influences him even in his stupor. The same thing is liable also to happen with almost all patients just before complete anæsthesia takes place, but the ether rarely requires to be suspended. Occasionally the respiration becomes embarrassed during the period of excitement, partly from the struggle itself, and partly perhaps from the increased flow of saliva, which is a common phenomenon

of etherization, or from the position of the tongue or head of the patient, and a condition may sometimes show itself characterized by lividity, rigidity, and convulsive motions of the extremities. These phenomena, it is an observation of Dr. H. J. Bigelow, of this city,* are in reality the tetanic symptoms which, Dr. Brown-Sequard has shown, precede the approach of asphyxia. Although alarming to the inexperienced, the state is in fact devoid of danger, provided the ether be momentarily suspended; this being done, the refusal to breathe soon gives place to a long-drawn inspiration, and in most instances complete insensibility immediately ensues. In such a case it is interesting to observe how readily the spasm yields, and how complete is the muscular relaxation which follows the free respiration of air unmixed with ether. It should therefore be borne in mind, that when there is muscular rigidity with lividity, the suspension of etherization will transform this into the relaxation of anæsthesia. Persons of intemperate habits succumb to ether slowly, and with greater reluctance and more opposition than persons unused to intoxication. The pulse should be watched by a competent person from the outset, and its failure, either in strength or frequency, lead to a more cautious use of the ether. It must, however, be remembered, that in experiments with anæsthetics upon animals, the heart has been found to be the *ultimum moriens*;† the respiratory movements, therefore, should not be forgotten or neglected, but any slowness or irregularity in the performance should at once receive attention. Attention has been drawn by Dr. H. J. Bigelow to the distinction between the effects of anæsthesia upon the pulse of the healthy subject suddenly reduced by accident, and a similar and even stronger pulse in a person, exhausted by long and grave disease. In the former case the vitality is unimpaired, and the pulse, even when hardly perceptible, rises with anæsthesia. Ether, therefore, is not to be withheld from a patient to be operated on, even in a state of collapse after severe accident, but great caution is demanded in its use with patients who are near death from chronic and exhausting disease, and who require operations‡. The best test of complete etherization is the snoring of the patient; and no operation, unless slight, should be undertaken until this symptom presents itself. The relaxation of the muscles of the extremities may occur without insensibility. The important distinction between *snoring* and *stertor* is, however, to be borne in mind. Whilst the former is caused only by the relaxation of the muscles of the palate, the latter arises from spasm of the vocal cords and partial closure of the rima glottidis, and thus becomes the immediate forerunner of the train of symptoms already referred to as indicative of partial asphyxia. Stertorous respiration demands, therefore, a brief suspension of inhalation; one or two inspirations of fresh air will, as already mentioned, almost instantly dispel the symptom. Ether may be administered to persons of all ages, from the new-born infant to the octogenarian. There is, however, a condition prone to manifest itself with children, especially those who are weak, strumous, or overgrown, which is due to its cumulative properties. It may show itself after almost any degree of etherization, and is characterized by a feeble pulse and slow respiration, not passing off with the readiness usually marking the phenomena of etherization. With young persons a cautious inhalation of five minutes will often induce an anæsthesia of half an hour, an effect wholly out of proportion to what the same amount of ether would produce in an adult. This state is not a dangerous one, and only requires time to dissipate its symptoms. Compression of the chest will expel the fumes of ether being eliminated from the pulmonary surface, and admit the entrance of a fresh supply of oxygen to stimulate the circulation. The inhalation should therefore be suspended at short intervals with children, and but little ether given at a time. It should also undoubtedly be used with great caution with persons, past the middle age of life, of

* Unpublished Records of the Boston Society for Med. Improvement.

† Du rôle de l'alcool et des anæsthesiques dans l'organisme, Lallemand, Perrin et Duroy, Paris, 1860, p. 298.

‡ Unpublished Records of the Boston Society for Med. Improvement.

such a general obesity or constitutional condition as may lead to the supposition of a fatty degeneration of the heart. In none of the alleged deaths from ether is there any mention, however, of valvular disease of the heart being found. Of this, then, and of any bad effect upon pulmonary affections, there need be no fear, for we see it constantly administered without detriment to persons more or less advanced in phthisis, for the common operation of fistula in ano. Its subsequent effects are rarely disagreeable. The nausea and vomiting which follow the use of any anæsthetic may be prevented or diminished by giving it upon an empty stomach. Faintness, although a rare event, is occasionally noticed, and demands the ordinary treatment by stimulants. Headache sometimes remains for a few hours, but seldom persists into the following day. We now and then hear of delirium, debility, and the non-return of a full use of the mental faculties, as temporary accidents from the use of ether.* Such occurrences must be of extreme rarity, and probably find their explanation as much in the idiosyncrasies of patients as in the effects of the anæsthetic.

II. Having thus detailed what we conceive to be the conditions of its successful and safe administration, we undertake, in the second place, to prove that, these conditions being fulfilled, sulphuric ether is, of all anæsthetic agents, alone worthy of unlimited confidence. Confirmation of this assertion is to be found in what we know of the use of ether in other places. It is true that thus far this has been limited to a few localities, but wherever it has been adopted the confidence and freedom with which it is administered are worthy of notice. In 1857, it was stated that for about eight years ether alone had been used in the civil or hospital practice of Lyons, in France, and that during that time the necrology of anæsthetics, so far at least as that city was concerned, had remained closed.† And here, in Boston, where more ether has probably been inhaled during the last fifteen years than in any other place in the world, from the time when Dr. George Hayward performed the first capital operation under its influence (Nov. 7, 1846) down to the present day, no fatal result has ever occurred, or been heard of in the vicinity, though repeated deaths have happened from chloroform during the same period. But to sustain the above conclusion with regard to the absolute safety of sulphuric ether, your Committee place their chief reliance upon the histories of the recorded fatal cases thought to have been caused more or less by its inhalation, and upon the result of their own efforts to obtain information of all others of the kind known to the profession anywhere. With unequalled facilities to examine the literature of the subject under discussion, with all the chief foreign and American Journals at hand, and the results of a most extensive distribution of circulars before us, no case of which we have knowledge can be cited as unquestionably and unavoidably fatal from the breathing of pure sulphuric ether.‡

The following two conditions must be considered essential to any case of death fairly attributable to the inhalation of an anæsthetic agent:—1st, That the event should occur while the patient is actually in an anæsthetic state. 2d, That the circumstances of its occurrence should be inexplicable by any phenomena of disease or operation. Such a death should be unavoidable by any precautions which might be adopted were the patient to be again rendered insensible under similar circumstances. It must consequently be sudden and unexpected in manifesting its symp-

toms, as well as rapid in its progress to a termination. The unmistakable deaths following the use of chloroform have usually been almost instantaneous; out of twenty-seven which occurred in ten minutes, fifteen took place in less than two minutes.* No conclusive light can be thrown on the subject by a post-mortem examination; it can only demonstrate a cause exculpating the anæsthetic—there being no pathognomonic signs of death from the use of these agents. It is clearly unreasonable, therefore, to attribute to anæsthetics deaths happening long after patients have recovered from their immediate and specific influence. A man is etherized for lithotomy, and dies of pneumonia a week later; or, a female, anæmic and feeble, suffering from the constitutional effects of a malignant tumor of rapid and large growth, inhales chloroform for the bloody or tedious operation by which it is to be removed, and dies twenty-eight hours afterwards without rallying. Events of this description are not so infrequent where no anæsthetic has been used as to require any other explanation than such as may be found in the operation or disease itself, and are obviously liable to occur from accidental causes under any circumstances. Yet this is the character of a large proportion of the facts cited by writers as evidence to prove the occasional occurrence of fatal results from the inhalation of ether. Of the whole number of alleged deaths from sulphuric ether (forty-one) which has been collected by your Committee, sixteen survived the inhalation from three to sixteen days, and eight from three to fifteen hours. In all of these death occurred after the peculiar primary effects of its use had subsided, from a secondary set of symptoms, which were either simply coincident or else such as are well known frequently to terminate in death when no anæsthetic has been used, and which, moreover, never show themselves in cases of inhalation for slight and trivial operations where the primary effects of ether have been just as well marked as in the severer operations after which they were alone noticed. Of the six cases in which death occurred in less than twenty-four hours, three, viz. Nos. 3, 8, and 12, have been almost universally set aside as inconclusive; two, Nos. 15 and 21, are manifestly unattributable to the ether. Of the sixth, No. 40, the details are very meagre, but that the death had any connexion with the anæsthetic, is at least improbable. In the remaining seventeen cases where death was immediate, or nearly so, the connexion between the result and the inhalation is either problematical or else manifestly absurd and unfounded, except in four instances, viz. cases 1, 25, 34, and 39, where it was due to asphyxia, brought about by wholly avoidable causes. The administration of an anæsthetic *in articulo mortis*, as for example in an operation for strangulated hernia, may sometimes merge the sleep which it produces into the sleep of death, without the termination of the case being hastened by, or attributable to, the inhalation. Patients die from *croup* during an attempt to save life by tracheotomy, and may equally succumb to tetanus or delirium tremens during the inhalation of ether or chloroform. Cases 23, 27, 28, and 30, must be considered as instances belonging to such a category. Of the seven cases in which the period when death took place is not mentioned, there is no one where the evidence does not admit of strong doubt as to the connexion between the result and its cause, or else is insufficient to produce conviction. The remaining cases of the table not yet accounted for, viz. Nos. 16, 19, 20, 24, and 29, are no better able to stand the test of examination. The character and circumstances of the operation in the first three certainly absolve the ether. In the fourth (No. 24), the nature of the injury as shown by the autopsy, and the condition of the patient at the time of inhalation—it being such as in the unanimous opinion of those present would not permit the use of chloroform, together with the fact that he never was fairly etherized—point to some other cause than the anæsthetic to account for the fatal result. The history of, and the statements connected with, the remaining case (No. 29),

* Lente, N. Y. Journ. of Med., Nov., 1856. Clark, do. do., Sept., 1856. Hooker, Boston Med. and Surg. Journ., Vol. 53, p. 231. Humphry, Provincial Med. and Surg. Journ., Aug. 9, 1848.

† Revue Médicale, 1857, p. 602.

‡ In an appendix to this report, every instance of alleged death, or allusion to such, caused by any form of ether, which we have been able to find, is given with all the important facts of its occurrence, or at least so far as they could be obtained. If any other cases, conclusive or not, have occurred, this Committee is not responsible for their ignorance of them, as they have used every means in their power by notices in newspapers and Medical Journals, and by a correspondence scattered over the United States (long prior to the interruption of mail communication by the rebellion), British Possessions, West Indies, England, and Europe, to collect all existing facts bearing on the subjects of their investigations.

* British Med. Journal, Feb. 21, 1857.

equally exclude that from being relied on as an instance of death from ether. The statements of any author, however distinguished in position, not accompanied by proof in the form of *pièces justificatives*, must remain of no value in face of the direct evidence of your Committee, that their careful search of journals and monographs furnishes not a single conclusive case of death from the proper inhalation of pure sulphuric ether.

III. In contrast with the foregoing evidence, how striking is the admission of the staunchest partisans of chloroform, that no care on the part of the administrator, nor intrinsic chemical perfection, will insure the safety of the person breathing its vapor! Neither the skill of a Dr. Snow, nor the laboratory of Duncan, Flockhart & Co., appears to exempt those who inhale chloroform from the fatal calamities which sometimes ensue wherever it has been used. In 1857, in a discussion before the Academy of Medicine, M. Ricord spoke of the use of chloroform as "an accident which complicated an operation;" and in 1859, the President of the Paris Société de Chirurgie, M. Hervez de Chegoïn, seriously proposed the question, "Whether its use had not better be actually suspended until some method of using it with constant security had been discovered, or, if it is to remain of so uncertain safety, even renounced altogether?"* In 1856, Mr. Erichsen, of London, in a letter to Dr. S. D. Townsend, of this city, said, that "when a patient was fully under the influence of chloroform he was on the verge of death."† The epithet "*steau chloroformique*" is, therefore, no undeserved one, for in any man's hands chloroform may indeed become a scourge whose blows shall fall so suddenly and mysteriously, that before the surgeon's knife is taken up, the patient's life may have passed away beyond resuscitation. No such impressions have ever prevailed with regard to sulphuric ether. No one can die from it as he may die from chloroform. Dr. J. C. Dalton, in a letter to your Committee, speaking of the use of chloroform in the vivisection of animals, says, "I am convinced from my experience, that no caution will prevent its producing a fatal effect, and no care will enable the operator to see when the danger is threatened." On the other hand, with regard to ether, he states, "I never feel any anxiety as to the safety of an animal under etherization, provided I can myself watch the state of the pulse and respiration, or can rely upon the aid of a competent assistant for that purpose: So far as my observation goes, the dangerous symptoms in the case of ether can always be recognised, with ordinary care, in sufficient time to prevent a fatal result." Dr. John Snow declares that "he holds it almost impossible that a death from ether can occur in the hands of a medical man who is applying it with ordinary intelligence and attention.‡

The more agreeable odor, the more rapid result, and the smaller bulk, are the only compensations offered as an offset to the suspended sword which thus hangs over the surgeon whenever he invokes the aid of chloroform. The first of these advantages seems too unimportant to be serious; nor are all people of one opinion as to the more agreeable smell of chloroform. M. Roux talks of its "nauseating and sickly sweet odor," as being more painful to inhale than that of ether;§ and M. Sédillot says, that of patients submitted by him "sometimes to the use of ether and sometimes to that of chloroform, all have preferred ether."¶ Dr. Snow esteems the odor of, and the sensations produced by, ether as much more pleasurable than those of chloroform.‡ Then, too, as to rapid action, a patient may be put by ether into a thorough anæsthetic condition for the performance of a by no means short or trivial operation in one minute and a

third.* Chloroform can hardly do more than that. Is the rapid production of anæsthesia, however, a desirable thing? It is an assertion based upon statistics that the early stage of chloroformization is the most dangerous.† The agitation and excitement of patients during the first moments of inhalation may explain this, as the rapidity and intensity of anæsthesia are in proportion to the activity of respiration and circulation. Such being the case, a gentler and slower anæsthetic than chloroform ought to carry the patient more safely over this dangerous period, by allowing the etherized blood gradually to penetrate the remotest parts of the system, and thus avoid the prostration of a sudden and violent impression upon the nervous centres. The necessary duration of the anæsthetic condition, when gradually induced, must for the same reason be more satisfactorily ensured. A rapid anæsthesia, although complete, is apt to be of very short duration, and the patient may recover his sensibility as suddenly as he lost it. This does not occur so frequently when the anæsthetic has taken effect in a slower manner, and may be explained by supposing that a volume of the blood first charged in the lungs passes to the brain and narcotizes the patient, and that the blood which remains in the extremities, not yet touched by the vapor, will, if the process be arrested, in its turn flow through the brain and at once revive him. On the other hand, a more protracted inhalation, such as is usually the case with ether, ensures the gradual saturation of the whole circulation. Here, too, is an additional illustration of the important statement before made, that unless an operation is to be short, the surgeon should not be content with the appearance of the first symptoms of insensibility, but push the anæsthesia till the patient snores.

The advantages of chloroform in respect to portability are of little consequence in civil practice. But when an agent so much more compact than ether can be used in military hospitals and on the battle-field, the necessity of reducing baggage to its minimum demands, it has been alleged, that the less bulky anæsthetic be preferred, and this argument is usually strengthened by a reference to the results of its use in the Crimea, viz. two deaths in 30,000 cases, one in the French and one in the English army. These statistics, apparently so conclusive, will not, as your Committee believe, stand the test of examination. How was it possible to obtain accurate information from every battery, rifle pit, or trench, where chloroform was given? What surgeon would not, under the circumstances in which it must constantly have been administered, be liable often to attribute to the effects of an injury fatal results really due to the anæsthetic—especially if it is true, as Dr. Snow states in a communication to Mr. Guthrie,‡ that to "take 10 minims of chloroform into the lungs when insensibility is almost complete, must be attended with danger." A perusal of the letter of Dr. J. Hall, Inspector General of Hospitals in the Crimea, to the Director General at London,§ or of a paper by Dr. Monat, Deputy Inspector General in the English Army, read by him in the Crimea to the Crimean Medical Society,|| will persuade the reader that more than one death from chloroform occurred during the war, amongst the British troops. Such certainly is that gentleman's impressions, and it does not appear to have been either opposed or contradicted by the other members of the Crimean Society. Dr. Lente says that the assertion of Baudens, that only one fatal case happened amongst the French from chloroform, is denied by other surgeons, who themselves saw deaths occur from its use.¶ But whether this is true or not, that 30,000 soldiers should escape the dangers of chloroform, is no argument in its favor. It is well known that a vast number of missiles are thrown in battle without touching a single person, yet no one would pre-

* Seance du Mai, 1859.

† See Record of the Boston Society for Med. Improvement, Vol. 2, p. 34.

‡ On Chloroform and other Anæsthetics, their Action and Administration, London, 1858, p. 362.

§ L'Union Médicale, Jan'y 4, 1848.

¶ De l'Insensibilité produite par le Chloroforme et par l'Ether, et des Opérations sans douleur, Paris, 1848. p. 95.

¶ Loc. cit., p. 357.

• British and Foreign Med. Chir. Review, October, 1859, p. 552.

† Med. Times and Gazette, May 12, 1860.

‡ Commentaries, London, 1855, p. 39.

§ Med. and Surg. History of the British Army which served in Turkey and the Crimea during the War against Russia. "Blue Book," p. 269.

¶ Med. Times and Gazette, Aug. 30, 1856.

¶ American Journal of Medical Sciences, April, 1861.

tend that this fact diminishes in the slightest degree the danger in the flight of a solitary bullet. The position of chloroform is precisely identical. The amount of ether required for army use, if properly administered and economized, is not very great. The quantity necessary for a regiment, especially one with easy access to its supplies, cannot add much encumbrance to the stores of the hospital department. At all events, it might be used to a certain extent, and the soldier's life in a degree secured against the treachery of one foe not less dangerous than the bullets of the enemy. The objection to ether on account of its inflammability does not apply with any more force to its use in armies than in private practice, and ordinary precautions will provide against accident from this cause. The more trivial the operation for which chloroform is inhaled, the more care should be taken in its use, fully two thirds of the deaths from its effects having occurred during the performance of minor operations.* The very opposite of this is true of ether. Only after long, protracted inhalation, during operations accompanied by great loss of blood, or involving great prostration of the general system, can any possible anxiety be felt. The friends of chloroform admit that "over 150 deaths" have already occurred from its use.† This, it is urged, is only about 11½ per year since its first application in 1848. Had as many fatal cases happened in that period from opium, aconite, arsenic, strychnia, or other poisonous drugs, administered by regular physicians, would the use of such agents still be authorized? And how much stronger would the case be, did they produce death in the unexpected and sudden manner in which chloroform strikes its victims! The objections to chloroform apply with equal force to chloric ether, which is chloroform diluted with alcohol, to amylene, and to the mixture of sulphuric ether and chloroform, in whatever proportion. The dangers of this last are well shown in a case reported in the Appendix, where a boy 5 years of age died within three or four minutes after breathing a mixture of four parts of ether and one of chloroform. The addition of chloroform to ether being unnecessary, only renders dangerous an anæsthetic which is otherwise safe, and is liable to lead to a carelessness in its administration which would not occur with chloroform, and might prove as dangerous as when that anæsthetic is alone used in an unadulterated state‡. Of the new agent, "kerosolene," recently discovered in this city, the remarkable physical properties of which are so attractive, sufficient experience has not yet been had to authorize an opinion upon its future value.

Of the action of anæsthetics on the system, we have but an imperfect and inconclusive knowledge. The following statements are cited from the work of Lallemand, Perrin and Duroy, already referred to. This recent and elaborate treatise depends for its facts upon numerous experiments on animals, and upon such carefully conducted researches as entitle it to confidence. 1st. Anæsthetics are neither transformed nor destroyed in the system, but are rapidly eliminated from it, chiefly by the lungs, and to a limited extent by the cutaneous surface. Chloroform and amylene being insoluble in water, no traces of them are ever found in the urine. Sulphuric ether being more soluble, a small

quantity of this may be detected by re-agents in the renal secretion. 2d. The blood, and the organs of animals dead from *etherism* (the name given to this special intoxication by the above-named authors) contain the anæsthetic agent employed, the presence of which is easily determined by special chemical research; and the following figures show in what proportion the principal viscera contain the anæsthetic, in reference, for each of the agents employed, to the quantity found in the blood.

SOURCE OF ANALYSIS.	CHLOROFORM.	SULPH. ETHER.	AMYLENE.
Blood,	1.00	1.00	1.00
Cerebral substance,	3.92	3.25	2.06
Liver,	2.08	2.25	1.00
Muscular tissue,	0.16	0.25	Traces.

From this it appears that anæsthetics accumulate in the cerebro-spinal system. 3d. It is not easy to explain the deaths from chloroform and amylene, or from ether (as sometimes seen in the lower animals), but it would seem probable, both from the phenomena which they present and the experiments which have been made, that they are the consequence of an abolition of the functions of the nervous system, and not of asphyxia.

The general conclusions which have been arrived at by your Committee may be summed up as follows:—1st. The ultimate effects of all anæsthetics show that they are depressing agents. This is indicated both by their symptoms and by the results of experiments. No anæsthetic should therefore be used carelessly, nor can it be administered without risk by an incompetent person. 2d. It is now widely conceded, both in this country and in Europe, that sulphuric ether is safer than any other anæsthetic, and this conviction is gradually gaining ground. 3d. Proper precautions being taken, sulphuric ether will produce entire insensibility in all cases, and no anæsthetic requires so few precautions in its use. 4th. There is no recorded case of death, known to the Committee, attributed to sulphuric ether, which cannot be explained on some other ground equally plausible, or in which, if it were possible to repeat the experiment, insensibility could not have been produced and death avoided. This cannot be said of chloroform. 5th. In view of all these facts, the use of ether in armies, to the extent which its bulk will permit, ought to be obligatory, at least in a moral point of view. 6th. The advantages of chloroform are exclusively those of convenience. Its dangers are not averted by its admixture with sulphuric ether in any proportions. The combination of these two agents cannot be too strongly denounced as a treacherous and dangerous compound. Chloric ether, being a solution of chloroform in alcohol, merits the same condemnation.

R. M. HODGES, C. T. JACKSON,
GEO. HAYWARD, J. BAXTER UPHAM,
S. H. TOWNSEND,

Committee.

The foregoing report was accepted, and its conclusions adopted by the Society.

FRANCIS MINOT, Secretary.

Dr. C. T. Jackson, one of the Committee, objects and excepts to the clause in this report in which "all mixtures of ether and chloroform" are denounced; viz. to the words, "the dangers of chloroform are not averted by admixture with sulphuric ether," and to the terms, "treacherous and dangerous compound" of chloroform and ether. He believes that a mixture of four measures of ether and one measure of chloroform may be employed without danger, or with very little danger, and that the risks from chloroform are diminished more than four fifths by this combination. He believes it to be necessary to have an anæsthetic agent of less bulk than ether, and not so dangerous as chloroform, for army uses, and is satisfied that this mixture, which he has employed and prescribed, completely answers the purposes required.

* British Med. Journal, Feb. 21, 1857. Med. Times and Gazette, May 12, 1860.

† Dublin Med. Press, June 5, 1861.

‡ It has been said that a mixture of ether and chloroform was employed with great success by the French in the Crimean war. There is no published documentary evidence to prove that such a combination was adopted. No mention is made of it by the Inspector-General at the head of the French Medical Service in the Crimea. On the contrary, a correspondence between that officer and the General-in-chief leaves no possible doubt as to the exclusive use of chloroform; his general remarks, also, render it certain that no other anæsthetic was substituted at any time during the war. (Relation Médico-Chirurgicale de la Campagne d'Orient. Paris, 1857. Pp. 123, 160, and 456.) In none of the Journals of the period, nor in the English Government Report of the Medical History of the War, is there any statement leading to the inference that a mixture of ether and chloroform was used by either army during that campaign, or anything which can authorize the supposition that the word "chloroform" is used as synonymous with "anæsthetic," and therefore includes in its meaning the combination said to have been employed. A point so important could not fail to be set forth in distinct terms in such careful reports as were made after the Crimean war.

TABLE OF ALLEGED DEATHS FROM THE INHALATION OF ETHER.

No.	Operator or Reporter.	Date.	Locality.	Sex.	Age.	Operation, or purpose for which it was administered.	Period after which Death occurred.	Probable Cause of Death.
1	Figuier.	1847	Auxerre, France.	M.	55	Cancer of breast.	During op.	Asphyxia.
2	Robbs.	1847	England.	F.	21	Tumor of thigh.	40 hours.	Shock and exhaustion.
3	Eastment.	1847	"	M.	11	Amputation of thigh.	3 hours.	Shock of accident and operation.
4	Stann.	1847	"	M.	52	Lithotomy.	50 hours.	Shock and exhaustion.
5	Taylor.	—	"	—	—	—	—	Case similar to preceding.
6	Robinson.	1847	"	M.	70	Amputation.	4 days.	Shock, exhaustion, age.
7	Kopecky.	1847	Vienna, Austria.	M.	—	Amputation of thigh.	6 days.	Pneumonia.
8	Roel.	1847	Madrid, Spain.	F.	50	Cancer of breast.	8 hours.	Shock and state of general health.
9	Kopecky.	1847	Vienna, Austria.	F.	—	Amputation of arm.	5 days.	Pleuritis.
10	Jobert.	1847	Paris, France.	F.	33	Cancer of breast.	16 days.	Erysipelas and bronchitis.
11	Jobert.	1847	"	F.	47	Amputation of thigh.	15 days.	—
12	Mendoza.	1847	Barcelona, Spain.	F.	60	Amputation of leg.	15 hours.	Shock of accident and operation.
13	Schuh.	1847	Vienna, Austria.	F.	26	Amputation of thigh.	4 days.	Pyæmia.
14	Kopecky.	1847	"	M.	—	Hæmatocele.	13 days.	"
15	R. D. Mussey.	1847	New Hamp., U. S.	M.	—	Lithotomy.	5 to 6 hours.	Shock.
16	Johnson.	1853	Virginia, U. S.	F.	—	Excsect. of entire lower jaw	During op.	Shock, hæmorrhage, &c.
17	Forbes.	1847	England.	Boy.	—	Lithotomy.	Many days.	Intense local inflammation.
18	Humphry.	1848	"	M.	—	Amputation of arm.	3 days.	—
19	Barrière.	1852	Lyons, France.	F.	55	Excision sup. maxilla.	During op.	—
20	De Ottingen.	1847	Dorpat, Russia.	M.	70	Amputation of thigh.	"	Shock.
21	Lewis.	1847	Boston, U. S.	M.	—	Amp. at shoulder joint.	12 hours.	Shock of accident and operation.
22	Richet.	1847	Paris, France.	M.	43	" " "	11 days.	Capillary bronchitis.
23	—	1860	New York, U. S.	—	—	Op. strang. hernia.	—	—
24	W. H. Mussey.	1860	Ohio, U. S.	M.	50	Surgical examination.	15 to 20 min.	Comm. frac. os inn. perhaps fat. deg. of heart.
25	—	1860	New York, U. S.	—	—	" "	During inhal.	Asphyxia?
26	Rigaud.	1847	Paris, France.	M.	63	" "	4 days.	Bronchitis.
27	Bassett.	1847	Alabama, U. S.	—	—	Actual cauterization.	During inhal.	Tetanus.
28	Roux.	1847	Paris, France.	—	—	Tetanus.	—	"
29	Clark.	1859	New York, U. S.	F.	27	Intense headache.	During inhal.	Disease of the brain.
30	—	1855	Boston, U. S.	M.	32	Delirium tremens.	"	Delirium tremens.
31	Piedaguel.	1847	Paris, France.	M.	—	—	15 days.	Arachnitis.
32	Eve.	1849	Kentucky, U. S.	M.	—	Exhilarating effects.	4 days.	Symptoms of meningitis.
33	Miller.	—	"	F.	15	" "	12 days.	—
34	Payne.	1851	London, Eng.	M.	—	As an anodyne.	—	Asphyxia.
35	Velpeau.	1847	Paris, France.	F.	60	Tumor of breast.	3 days.	Acute pulmonary disease.
36	Roux.	1847	"	—	—	Opening of an abscess.	3 days.	Exhaustion.
37	Roux.	1848	"	M.	82	Lithotomy.	—	—
38	Bergson.	1847	Berlin, Prussia.	M.	—	Lithotriety.	—	—
39	Velpeau.	1847	Paris, France.	—	—	Excision of tonsils.	—	Asphyxia.
40	Scoutetten.	—	Metz, Paris.	M.	50	—	4 hours.	—
41	Giraldes.	1860	—	—	—	—	—	—

ETHER AND CHLOROFORM COMBINED.

1	Crockett	1857	Virginia, U. S.	M.	5	Fatty tumor.	At end of op.
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ETHER AND SUBSEQUENTLY CHLOROFORM.

1	March.	1854	Albany, U. S.	F.	18	Tumor of neck.	2 hours.
2	Vallette.	1858	Lyons, France.	—	—	—	During inhal.

CHLORIC ETHER.

1	—	1853	Lynn, U. S.	F.	—	Extraction of tooth.	During inhal.
2	Haynes.	1852	N. Hamps., U. S.	F.	—	Tumor of thigh.	15 to 20 min.
3	Ingalls.	1852	Chelsea, U. S.	M.	20	Eversion of toe-nail.	During inhal.

CHLORIC ETHER AND CHLOROFORM.

1	Folitz.	1852	East Boston, U. S.	—	—	—	—
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RESUMÉ OF 41 CASES OF ALLEGED DEATH FROM SULPHURIC ETHER.

In 6 cases death occurred in from 11 to 16 days.	In 3 cases death occurred in from 3 to 6 hours.
In 9 " " " " 3 to 6 days.	In 1 " " " " from 15 to 20 minutes.
In 1 " " " " after "many days."	In 4 " " " " during the operation.
In 2 " " " " from 40 to 50 hours.	In 4 " " " " inhalation.
In 3 " " " " from 8 to 15 hours.	In 8 " the time which elapsed is not stated.

TO CORRESPONDENTS.

L. B. C.—Communication received, and very acceptable; the subject will soon be taken up in detail.

E. P. and others.—We cannot give up our columns to the discussion of Homeopathy. The subject is thoroughly disgusting to the great mass of the profession.

E. A. (Yonkers, N. Y.)—The case reported illustrates well conservative surgery, and will appear next week.

J. T. R. (Fairfield, O.)—Paper received and will appear.

Dr. Kidd, (London)—Pamphlet received.

J. K. (Onondaga, N. Y.)—You will receive a letter.

PUBLICATIONS RECEIVED.

Placenta Prævia: its History and Treatment. By William Reade, M.D. Philadelphia: J. B. Lippincott & Co., 1861. pp. 340.

The Causes and Treatment of Imperfect Digestion. By Arthur Leared, M.D. Second Edition. London: Churchill, 1861. pp. 218.

On the Sounds caused by the Circulation of the Blood. By Arthur Leared, M.D. London: Churchill, 1861. pp. 22.

METEOROLOGY AND NECROLOGY OF THE WEEK IN THE CITY AND COUNTY OF NEW YORK.

From the 21st day of October to the 28th day of October, 1861.

Abstract of the Official Report.

Deaths.—Men, 59; women, 81; boys, 136; girls, 100—total, 401. Adults, 165; children, 236; males, 220; females, 181; colored, 7. Infants under two years of age, 176. Children reported of native parents, 84; foreign, 164.

Among the causes of death we notice:—Apoplexy, 7; Infantile convulsions, 31; croup, 5; diphtheria, 4; scarlet fever, 14; typhus and typhoid fevers, 8; cholera infantum, 15; cholera morbus, 1; consumption, 63; small-pox, 4; dropsy of head, 17; infantile marasmus, 26; diarrhoea and dysentery, 16; inflammation of brain, 14; of bowels, 8; of lungs, 24; bronchitis, 6; congestion of brain, 11; of lungs, 3; erysipelas, 0; whooping cough, 4; measles, 3. 212 deaths occurred from acute disease, and 45 from violent causes. 280 were native, and 121 foreign; of whom 2 came from Ireland; 3 died in the Immigrant Institution, and 43 in the City Charities; of whom 15 were in the Bellevue Hospital.

Abstract of the Atmospheric Record of the Eastern Dispensary, kept in the Market Building, No. 57 Essex street, New York.

1861	Barometer.		Temperature.			Difference of dry and wet bulb. Therm.		W. Ind.	Mean amount of cloud.	Humidity Sat'n, 1000
	Mean height.	Daily range.	Mean.	Min.	Max.	Mean.	Max.			
	IN.	IN.	°	°	°	°	°			
27th.	29.17	.16	50	42	58	9	14	N. W.	.09	679
28th.	29.00	.20	44	36	51	8	12	N.	0	540
29th.	29.70	.44	50	39	60	10	16	N.W. to S.	0	479
30th.	29.47	.14	52	44	60	7	10	N. to S.	4	607
31st.	29.59	.47	53	45	60	8	12	N.W. to S.W.	0	561
1st.	29.14	.54	54	44	63	7.5	11	N. to S.	5.5	557
2d.	29.64	.58	58	48	69	1	8	N.E. to S.E.	10	811

REMARKS.—27th. Light rain, early A.M.; wind fresh all day. 28th. Wind fresh all day. 30th. Very light rain at intervals during the day; sky variable. 1st. Fog, at sunrise; cloudy P.M. 2d. Fresh wind A.M.; light rain after 8 A.M.; great storm of wind and rain, P.M. Rain fell 3.25 in.

NOTICE.—The rain fall of last week printed 61 and 31 in. should be respectively, 61 and 31 in.

MEDICAL DIARY OF THE WEEK.

Monday, Nov. 11.	NEW YORK HOSPITAL, Dr. Buck, half-past 1 P.M.
	BELLEVUE HOSPITAL, Dr. Loomis, Is. Hos., half-past 1 P.M.
Tuesday, Nov. 12.	NEW YORK HOSPITAL, Dr. Watson, half-past 1 P.M.
	BELLEVUE HOSPITAL, Dr. Clark, half-past 1 P.M.
	OPHTHALMIC HOSPITAL, 1 P.M.
Wednesday, Nov. 13.	NEW YORK HOSPITAL, Dr. Bulkley, half-past 1 P.M.
	BELLEVUE HOSPITAL, Dr. Sayre, Is. Hos., half-past 1 P.M.
	PATHOLOGICAL SOCIETY, half-past 7 P.M.
Thursday, Nov. 14.	NEW YORK HOSPITAL, Dr. Buck, half-past 1 P.M.
	BELLEVUE HOSPITAL, Dr. Elliot, half-past 1 P.M.
	OPHTHALMIC HOSPITAL, 1 P.M.
Friday, Nov. 15.	NEW YORK HOSPITAL, Dr. Watson, half-past 1 P.M.
	BELLEVUE HOSPITAL, Dr. Flint, half-past 1 P.M.
	EYE INFIRMARY, Dr. Noyes, half-past 1 P.M.
Saturday, Nov. 16.	NEW YORK HOSPITAL, Dr. Bulkley, half-past 1 P.M.
	BELLEVUE HOSPITAL, Dr. Parker, half-past 1 P.M.
	Dr. Wood's Clinic, half-past 2 P.M.
	OPHTHALMIC HOSPITAL, 1 P.M.

SPECIAL NOTICES.

NEW YORK COUNTY MEDICAL SOCIETY.—*The Anniversary Meeting of the New York County Medical Society will be held at the College of Physicians and Surgeons, corner of 23d st. and 4th Avenue, on Monday next, the 11th inst., at 7½ o'clock P.M. By order of HENRY S. DOWNS, M.D., Secretary.*

A Work on Mal-Practice,

(PREPARING.)

MEDICAL JURISPRUDENCE,

IN ITS APPLICATION TO THE PRACTICE OF MEDICINE, SURGERY, AND MIDWIFERY, IN THE UNITED STATES.

By STEPHEN SMITH, M.D.,

Professor of the Principles and Practice of Surgery in the Bellevue Hospital Medical College.

In no country is the practitioner of Medicine, Surgery, and Midwifery so frequently arraigned in courts of law for alleged mal-practice, and his treatment of disease made the subject of litigation, as in our own. Within the past few years, this branch of Medical Jurisprudence has become so rapidly developed that it is now beginning to assume an importance of the deepest interest to the profession.

It is with a view to illustrate the legal responsibilities of the medical practitioner, in his several capacities as Physician, Surgeon, and Accoucher, that the preparation of this treatise has been undertaken. It will be based on the legal evidence which has long been accumulating in our courts, and which must remain unavailable to both the medical and legal profession, until reduced in a systematic work. By patient and persevering effort for upwards of ten years, a large amount of material has been collected, embracing carefully prepared reports of about five hundred trials for alleged mal-practice, which completely illustrate the legal responsibilities of medical men in every branch of practical medicine, surgery, and obstetrics. Though the author is engaged in reducing this large amount of material to the form of a systematic treatise, he still solicits the co-operation of the profession in obtaining reports of trials for alleged mal-practice, in order that the collection may embrace all the litigated cases in this country, as well as the most recent judicial decisions. He, therefore, respectfully solicits from those who have cognizance of cases,

THE NOTES AND CHARGE OF THE PRESIDING JUDGE IN SUCH SUIT; OR THE NOTES OF LEGAL GENTLEMEN ENGAGED IN THE SUIT.

If these documents are not accessible, such facts as can be obtained of cases where suits for mal-practice have been tried, or instituted and quashed, or even threatened? The points of particular interest are—1. Names of parties to the suit; Court in which case was tried; Presiding Judge. 2. Date of trial. 3. History of the case in the treatment of which mal-practice was alleged, as nature of disease, injury, &c. complications, treatment, results, &c. 4. Testimony brought forward on the trial; opinions of experts, &c. 5. Opinions and Charge of Judge. 6. Verdict, &c. &c.

The strictest confidence will be observed in regard to names, facts, &c., communicated, and no use whatever will be made of them except in the preparation of this work. All communications will be duly acknowledged.

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